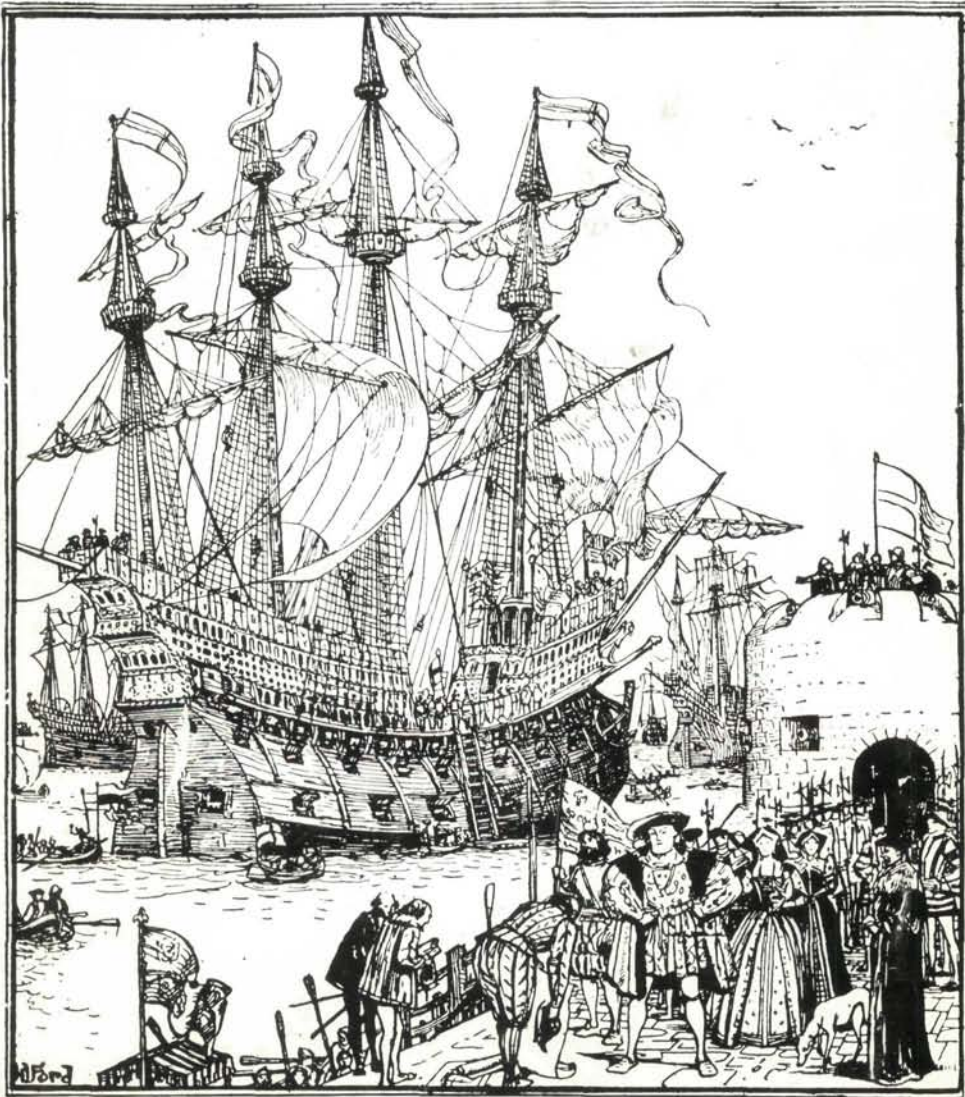


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Contents

VOLUME 87 • NUMBER 2 • APRIL 1982

Articles

- The Limits and Divisions of British History: In Search of the Unknown Subject,
BY J. G. A. Pocock 311
- Thomas Jefferson: The Virtue of Aesthetics and the Aesthetics of Virtue,
BY Lee Quinby 337
- Comment la Politique Vint aux Paysans: A Second Look at Peasant Politicization,
BY Eugen Weber 357

AHR Forum

- Insiders and Outsiders in American Historical Narrative and American History,
BY R. Laurence Moore 390
- Comments:* Edwin S. Gaustad, Gene Wise 413
- Reply:* R. Laurence Moore 420

Reviews of Books

GENERAL

- Eldon J. Eisenach. *Two Worlds of Liberalism: Religion and Politics in Hobbes, Locke, and Mill.*
By Richard Schlatter 424
- Marjorie O'Rourke Boyle. *Christening Pagan Mysteries: Erasmus in Pursuit of Wisdom.*
By Donald J. Wilcox 424
- Judith A. Merkle. *Management and Ideology: The Legacy of the International Scientific Management Movement.*
By Melvin Kranzberg 425
- A. J. H. Latham. *The Depression and the Developing World, 1914-1939.* By C. P. Kindleberger 426
- E. L. Jones. *The European Miracle: Environments, Economies, and Geopolitics in the History of Europe and Asia.*
By William Woodruff 426
- ANCIENT
- Bennett Simon. *Mind and Madness in Ancient Greece: The Classical Roots of Modern Psychiatry.*
By Valerie French 427
- T. Leslie Shear, Jr. *Kallias of Sphettos and the Revolt of Athens in 286 B.C.* By Samuel Eddy 428
- Joseph Fontenrose. *The Delphic Oracle: Its Responses and Operations with a Catalogue of Responses.*
By W. G. Forrest 428

JOAN M. FRAYN. <i>Subsistence Farming in Roman Italy</i> . By D. Brendan Nagle	429	ESTHER S. COPE. <i>The Life of a Public Man: Edward, First Baron Montagu of Boughton, 1562–1644</i> . By Retha M. Warnicke	445
RONALD MARTIN. <i>Tacitus</i> . By Herbert W. Benario	430	CLIVE HOLMES. <i>Seventeenth-Century Lincolnshire</i> . By David Underdown	446
ANTHONY R. BIRLEY. <i>The Fasti of Roman Britain</i> . By Robert K. Sherk	430	J. C. DAVIS. <i>Utopia and the Ideal Society: A Study of English Utopian Writing, 1516–1700</i> . By Arthur B. Ferguson	446
Z. RUBIN. <i>Civil-War Propaganda and Historiography</i> . By G. M. Paul	431	CORINNE COMSTOCK WESTON and JANELLE RENFROW GREENBERG. <i>Subjects and Sovereigns: The Grand Controversy over Legal Sovereignty in Stuart England</i> . By Stephen D. White	447
WALTER GOFFART. <i>Barbarians and Romans, A.D. 418–584: The Techniques of Accommodation</i> . By Walter Emil Kaegi, Jr.	432	J. R. JONES. <i>Britain and the World, 1649–1815</i> . By Charles R. Ritcheson	448
MEDIEVAL		MARIE PETERS. <i>Pitt and Popularity: The Patriot Minister and London Opinion during the Seven Years' War</i> . By Thomas W. Perry	449
GEORGES DUBY. <i>The Age of the Cathedrals: Art and Society, 980–1420</i> . Translated by ELEANOR LEVIEUX and BARBARA THOMPSON. By Penelope D. Johnson	432	GEORGE SHELTON. <i>Dean Tucker and Eighteenth-Century Economic and Political Thought</i> . By Stephen Baxter	449
WARREN T. TREADGOLD. <i>The Nature of the Bibliotheca of Photius</i> . By John P. Cavarinos	433	MICHAEL TURNER. <i>English Parliamentary Enclosure: Its Historical Geography and Economic History</i> . By Donald N. McCloskey	450
MALCOLM VALE. <i>War and Chivalry: Warfare and Aristocratic Culture in England, France, and Burgundy at the End of the Middle Ages</i> . By John Bell Henneman	433	JOHN ROACH. <i>Social Reform in England, 1780–1880</i> . By F. David Roberts	451
MARJORIE CHIBNALL, ed. <i>The Ecclesiastical History of Orderic Vitalis</i> . Vol. 1, <i>General Introduction, Books I and II (Summary and Extracts), Index Verborum</i> . By A. Daniel Frankforter	434	KAREL WILLIAMS. <i>From Pauperism to Poverty</i> . By Anthony Brundage	451
MARTIN WEINBAUM, ed. <i>The London Eyre of 1276</i> . By Sue Sheridan Walker	434	DAVID VINCENT. <i>Bread, Knowledge, and Freedom: A Study of Nineteenth-Century Working Class Autobiography</i> . By Duane C. Anderson	452
BERYL ROWLAND, ed. and translator. <i>Medieval Woman's Guide to Health: The First English Gynecological Handbook</i> . By Nancy G. Siraisi	435	D. G. PAZ. <i>The Politics of Working-Class Education in Britain, 1830–50</i> . By William J. Baker	453
COLIN RICHMOND. <i>John Hopton: A Fifteenth-Century Suffolk Gentleman</i> . By Joel T. Rosenthal	436	CHARLES MORE. <i>Skill and the English Working Class, 1870–1914</i> . By Frank Hearn	453
ELIZABETH M. HALLAM. <i>Capetian France, 987–1328</i> . By Andrew W. Lewis	436	DAVID CANNADINE. <i>Lords and Landlords: The Aristocracy and the Towns, 1774–1967</i> . By J. D. Marshall	454
MARINA WARNER. <i>Joan of Arc: The Image of Female Heroism</i> . By Anne Llewellyn Barstow	437	RICHARD A. COSGROVE. <i>The Rule of Law: Albert Venn Dicey, Victorian Jurist</i> . By Melvin Richter	455
NANCY G. SIRAI. <i>Taddeo Alderotti and His Pupils: Two Generations of Italian Medical Learning</i> . By Nicholas H. Steneck	438	CATHERINE CLINE. <i>E. D. Morel, 1873–1924: The Strategies of Protest</i> . By L. P. Carpenter	456
MODERN EUROPE		JO VELLACOTT. <i>Bertrand Russell and the Pacifists in the First World War</i> . By Steven E. Werner	457
OWEN CHADWICK. <i>The Popes and European Revolution</i> . By Charles H. O'Brien	438	STEPHEN ROSKILL. <i>Admiral of the Fleet Earl Beatty, the Last Naval Hero: An Intimate Biography</i> . By Robin Higham	457
ARNO J. MAYER. <i>The Persistence of the Old Regime: Europe to the Great War</i> . By Edward R. Tannenbaum	439	MARK SWENARTON. <i>Home Fit for Heroes: The Politics and Architecture of Early State Housing in Britain</i> . By Jose Harris	458
PAOLA BRUNDU OLLA. <i>L'Equilibrio difficile: Gran Bretagna, Italia e Francia nel Mediterraneo, 1930–1937</i> . By William C. Askew	440	JOHN MACNICOL. <i>The Movement for Family Allowances, 1918–45: A Study in Social Policy Development</i> . By Jerry H. Brookshire	459
JOHN TERRAINE. <i>To Win A War: 1918, The Year of Victory</i> . By David R. Woodward	441	D. R. THORPE. <i>The Uncrowned Prime Ministers</i> . By Roger Schinness	459
JAMES D. WILKINSON. <i>The Intellectual Resistance in Europe</i> . By Anthony Esler	442	PAUL HAGGIE. <i>Britannia at Bay: The Defence of the British Empire against Japan, 1931–1941</i> . By Robert J. Gowen	460
MURIEL ST. CLARE BYRNE, ed. <i>The Lisle Letters</i> . By Stanford E. Lehmberg	442	WILLIAM L. MILLER. <i>The End of British Politics? Scots and English Political Behaviour in the Seventies</i> . By Michael Curtis	461
RICHARD M. WUNDERLI. <i>London Church Courts and Society on the Eve of the Reformation</i> . By Ralph V. Turner	444	CHRISTINA BEWLEY. <i>Muir of Huntershill</i> . By Roger L. Emerson	462
LELAND H. CARLSON. <i>Martin Marprelate, Gentleman: Master Job Throkmorton Laid Open in His Colors</i> . By Richard L. Greaves	444		

- ENID GAULDIE. *The Scottish Country Miller, 1700–1900: A History of Water-powered Meal Milling in Scotland*. By Ian Donnachie 462
- MICHAEL TIERNEY. Eoin MacNeill: Scholar and Man of Action, 1867–1945. ed. by F. X. MARTIN. By Joseph O'Brien 463
- ROBERT J. SEALY. *The Palace Academy of Henry III*. By Edmond M. Beame 464
- PAUL LAWRENCE ROSE. *Bodin and the Great God of Nature: The Moral and Religious Universe of a Judaiser*. By Edwin G. Ehmke 464
- EMMANUEL LE ROY LADURIE. *L'argent, l'amour, et la mort en pays d'oc; Précédé du roman de l'abbé Fabre, Jean-l'ont pris (1756)*. By Robert Forster 465
- JEAN-PIERRE GUTTON. *Domestiques et serviteurs dans la France de l'ancien régime*. By Martha Ellis François 466
- RODERICK PHILLIPS. *Family Breakdown in Late Eighteenth-Century France: Divorces in Rouen, 1792–1803*. By James F. Traer 467
- CHARLES B. PAUL. *Science and Immortality: The Éloges of the Paris Academy of Sciences, 1699–1791*. By Thomas L. Hankins 468
- ROBERT FOX and GEORGE WEISZ, eds. *The Organization of Science and Technology in France, 1808–1914*. By Arthur P. Molella 468
- MICHEL L. MARTIN. *Warriors to Managers: The French Military Establishment since 1945*. By Ezra N. Suleiman 469
- JEAN-NOËL JEANNENEY. *L'argent caché: Milieux d'affaires et pouvoirs politiques dans la France du XX^e siècle*. By William A. Hoisington, Jr. 470
- RICHARD L. KAGAN. *Lausuits and Litigants in Castile, 1500–1700*. By Mary Elizabeth Perry 470
- JOHN B. OWENS. *Rebellion, monarquía y oligarquía: Murciana en la época de Carlos V*. By William D. Phillips, Jr. 471
- HENRY KAMEN. *Spain in the Later Seventeenth Century, 1665–1700*. By Richard L. Kagan 472
- JOSEFINA CRUZ VILLALÓN. *Propiedad y uso de la tierra en la Baja Andalucía: Carmona, siglos XVIII–XX*. By Carla Rahn Phillips 472
- SOLANGE DEYON and ALAIN LOTTIN. *Les "Casseurs" de l'été 1566: L'iconoclasme dans le Nord de la France*. By M. H. Schneider 473
- JOHAN AALBERS. *De Republiek en de vrede van Europa: De buitenlandse politiek van de Republiek der Verenigde Nederlanden na de vrede van Utrecht (1713), voornamelijk gedurende de jaren 1720–1733. Part 1, Achtergronden en algemene aspecten* [The Republic and the Peace of Europe: The Foreign Policy of the United Provinces of the Netherlands after the Peace of Utrecht (1713), Chiefly during the Years 1720–33. Part 1, Backgrounds and General Aspects]. By Paul Rosenfeld 474
- ALBERT KERSTEN. *Buitenlandse zaken in ballingschap: Groei en verandering van een ministerie, 1940–1945* [The Dutch Foreign Office in Exile: Growth and Change of a Ministry, 1940–45]. By Pierre-Henri Laurent 475
- HANS CHR. JOHANSEN. *Dansk økonomisk politik i årene efter 1784. Vol. 2, Krigsfinansieringsproblemer, 1789–93* [Danish Economic Policy in the Years after 1784. Vol. 2, Military Finance Problems, 1789–93]. By Heinz E. Ellersieck 475
- MATTHIAS MEYN. *Die Reichsstadt Frankfurt vor dem Bürgeraufstand von 1612 bis 1614: Struktur und Krise*. By Gerald L. Soliday 475
- WINFRIED SEELIG. *Von Nassau zum Deutschen Reich: Die ideologische und politische Entwicklung von Karl Braun, 1822–1871*. By Paul R. Sweet 476
- HANNES SIEGRIST. *Vom Familienbetrieb zum Managerunternehmen: Angestellte und industrielle Organisation am Beispiel der Georg Fischer AG in Schaffhausen, 1797–1930*. By Kenneth Barkin 477
- W. L. GUTTSMAN. *The German Social Democratic Party, 1875–1933: From Ghetto to Government*. By Richard Breitman 477
- OLLI KAIKKONEN. *Deutschland und die Expansionspolitik der USA in den 90er Jahren des 19. Jahrhunderts: Mit besonderer Berücksichtigung der Einstellung Deutschlands zur spanisch-amerikanischen Krise*. By Woodruff D. Smith 478
- PETER D. STACHURA. *The German Youth Movement, 1900–1945: An Interpretative and Documentary History*. By Michael Stephen Steinberg 479
- RODERICK STACKELBERG. *Idealism Debased: From Völkisch Ideology to National Socialism*. By Herman Lebovics 479
- VOLKER WÜNDERLICH. *Arbeiterbewegung und Selbstverwaltung: KPD und Kommunalpolitik in der Weimarer Republik: Mit dem Beispiel Solingen*. By Dan S. White 480
- ERNST WILLI HANSEN. *Reichswehr und Industrie: Rüstungswirtschaftliche Zusammenarbeit und wirtschaftliche Mobilmachungsvorbereitungen, 1923–1932*. By Henry A. Turner 481
- ILSE MAURER and UDO WENGST, eds. *Politik und Wirtschaft in der Krise, 1930–1932: Quellen zur Ära Brüning*. By Michael Geyer 481
- PIERRE AYÇOBERRY. *The Nazi Question: An Essay on the Interpretations of National Socialism, 1922–1975*. Translated by ROBERT HURLEY. By Roderick Stackelberg 483
- JAMES M. RHODES. *The Hitler Movement: A Modern Millenarian Revolution*. By T. L. Brink 483
- FRIEDHELM GOLÜCKE. *Schweinfurt und der strategische Luftkrieg 1943: Der Angriff der US Air Force vom 14. Oktober 1943 gegen die Schweinfurter Kugellagerindustrie*. By Keith W. Bird 484
- BRADLEY F. SMITH. *The Road to Nuremberg*. By William J. Bosch 485
- KENDALL L. BAKER et al. *Germany Transformed: Political Culture and the New Politics*. By Peter H. Merkl 485
- FRANZ QUARTHAL. *Landstände und landständisches Steuerwesen in Schwäbisch-Osterreich*. By Charles Ingrao 486
- BRUCE F. PAULEY. *Hitler and the Forgotten Nazis: A History of Austrian National Socialism*. By Radomir Luza 487
- ERIC COCHRANE. *Historians and Historiography in the Italian Renaissance*. By Donald J. Wilcox 488
- MARIA LUISA CAVALCANTI. *Le relazioni commerciali tra il regno di Napoli e la Russia, 1777–1815: Fatti e teorie*. By Mary Philip Trauth 488
- PAOLA NOTARIO. *La vendita dei beni nazionali in Piemonte nel periodo napoleonico, 1800–1814*. By Benjamin F. Brown 489
- PIETRO STELLA. *Don Bosco nella storia economica e sociale, 1815–1870*. By Raymond Grew 490
- FRANCESCO BALLENTA. *Le Due Sicilie e l'Egitto nel secolo XIX: Contributo alla storia delle relazioni economiche internazionali*. By Armand Patrucco 491
- JOSIP ADAMČEK. *Agrarni odnosi u Hrvatskoj od sredine XV do kraja XVII stoljeća* [Agrarian Relations in Croatia from the Middle of the Fifteenth to the End of the Seventeenth Centuries]. By Toussaint Hočevar 492

- WOLF DIETRICH BEHSCHNITT. *Nationalismus bei Serben und Kroaten, 1830–1914: Analyse und Typologie der Nationalen Ideologie*. By Elinor Murray Despalatović 492
- T. BĂRBUȚĂ and N. BOCȘAN. *Independența româniei în opinia belgiană* [Rumanian Independence in Belgian Opinion]. By Glenn E. Torrey 493
- VENIAMIN CIOBANU. *Relațiile politice româno-polone între 1699 și 1848* [Rumanian-Polish Political Relations, 1699–1848]. By Paul E. Michelson 494
- STELIAN POPESCU-BOTENI. *Relații între România și S.U.A. până în 1914* [Relations between Rumania and the U.S.A. up to 1914]. By John C. Campbell 494
- GEORGE EM. MARICA. *Studii de istoria și sociologic culturii române ardelen din secolul al XIX-lea*. Vol. 3, *George Barițiu—Istoric* [Studies in the History and Sociology of Transylvanian Rumanian Culture in the Nineteenth Century, vol. 3, George Barițiu—Historian]. By Keith Hitchins 495
- JOSEPH HELD, ed. *The Modernization of Agriculture: Rural Transformation in Hungary, 1848–1975*. By Gabor P. Vermes 496
- MONIKA GLETTLER. *Pittsburg—Wien—Budapest: Programm und Praxis der Nationalitätenpolitik bei der Auswanderung der ungarischen Slowaken nach Amerika um 1900*. By Martin L. Kovacs 496
- N. N. PUKHLOV. *Pol'skoe rabochee dvizhenie, 1890–1904 gg.* [The Polish Workers' Movement, 1890–1904]. By Edward Zebrowski 497
- ROMAN WAPIŃSKI. *Narodowa demokracja, 1893–1939: Ze studiów nad dziejami myśli nacjonalistycznej* [National Democracy, 1893–1939: From the Study of the History of Nationalistic Thought]. By Edward D. Wynot, Jr. 498
- IA. N. SHCHAPOV. *Vizantiiskoe i iuzhnoslavianskoe pravovoe nasledie na Rusi v XI–XIII vv.* [The Byzantine and South Slavic Legal Heritage in Rus in the Eleventh to Thirteenth Centuries]. By Jack Kollmann 499
- A. IA. DEGTIAREV. *Russkaia derevnia v XV–XVII vekakh: Ocherki istorii sel'skogo rasseleniia* [The Russian Village in the Fifteenth to Seventeenth Centuries: Essays on the History of Rural Settlement]. By Lawrence Langer 499
- JOSEPH T. FUHRMANN. *Tsar Alexis: His Reign and His Russia*. By Alexander V. Muller 500
- A. P. KORELIN. *Dvorianstvo v poreformennoi Rossii, 1861–1904 gg.: Sostav, chislennost', korporativnaia organizatsiia* [The Dvorianstvo in Reformed Russia, 1861–1904: Composition, Number, and Corporate Organization]. By Terence Emmons 501
- REX REXHEUSER. *Dumawahlen und lokale Gesellschaft: Studien zur Sozialgeschichte der russischen Rechten vor 1917*. By Roberta T. Manning 501
- S. M. SAMBUK. *Politika tsarizma v Belorussii vo vtoroi polovine XIX veka* [Tsarist Policy in Belorussia in the Second Half of the Nineteenth Century]. By Stephan M. Horak 502
- V. IA. GROSUL. *Revoliutsionnaia Rossiia i Balkany, 1874–1883* [Revolutionary Russia and the Balkans, 1874–83]. By Barbara Jelavich 503
- ANDREW ROSSOS. *Russia and the Balkans: Inter-Balkan Rivalries and Russian Foreign Policy, 1908–1914*. By Edward C. Thaden 503
- NEIL HARDING. *Lenin's Political Thought*. Vol. 2, *Theory and Practice in the Socialist Revolution*. By Albert S. Lindemann 504
- MICHAEL KETTLE. *Russia and the Allies, 1917–1920*. Vol. 1, *The Allies and the Russian Collapse, March 1917–March 1918*. By Rex A. Wade 505
- TIMOTHY DUNMORE. *The Stalinist Command Economy: The Soviet State Apparatus and Economic Policy, 1945–53*. By V. N. Bandera 506
- JIRI VALENTA. *Soviet Intervention in Czechoslovakia, 1968: Anatomy of a Decision*. By Robert M. Slusser 506

NEAR EAST

- MAURICE LOMBARD. *Études d'économie médiévale*. Vol. 3, *Les textiles dans le monde musulman du VII^e au XII^e siècle*. By Armand O. Citarella 507
- DANIEL PIPES. *Slave Soldiers and Islam: The Genesis of a Military System*. By C. E. Bosworth 508
- KARL K. BARBIR. *Ottoman Rule in Damascus, 1708–1758*. By Halil Inalcik 509
- ANN MOSELY LESCH. *Arab Politics in Palestine, 1917–1939: The Frustration of a Nationalist Movement*. By William Ochsenwald 509
- YAACOV BAR-SIMAN-TOV. *The Israeli-Egyptian War of Attrition, 1969–1970: A Case-Study of Limited Local War*. By Bernard Wasserstein 510
- SHAHROUGH AKHAVI. *Religion and Politics in Contemporary Iran: Clergy-State Relations in the Pahlavi Period*. By Nikki R. Keddie 511

AFRICA

- STEPHEN BAIER. *An Economic History of Central Niger*. By Edward Reynolds 512
- GRAHAM CONNAH. *Three Thousand Years in Africa: Man and His Environment in the Lake Chad Region of Nigeria*. By G. S. P. Freeman-Grenville 512
- TIMOTHY C. WEISKEL. *French Colonial Rule and the Baule Peoples: Resistance and Collaboration, 1889–1911*. By Kenneth J. Perkins 513
- CHRISTOPHER M. ANDREW and A. S. KANYA-FORSTNER. *The Climax of French Imperial Expansion, 1914–1924*. By Raymond F. Betts 514
- THOMAS Q. REEFE. *The Rainbow and the Kings: A History of the Luba Empire to 1891*. By R. W. Beachey 514
- FREDERICK COOPER. *From Slaves to Squatters: Plantation Labor and Agriculture in Zanzibar and Coastal Kenya, 1890–1925*. By Thomas Spear 515
- BONAR A. GOW. *Madagascar and the Protestant Impact: The Work of the British Missions, 1818–95*. By James B. Wolf 516
- LEROY VAIL and LANDEG WHITE. *Capitalism and Colonialism in Mozambique: A Study of Quelimane District*. By Frederick Cooper 516
- GIDEON SHIMONI. *Jews and Zionism: The South African Experience, 1910–1967*. By Robert G. Weisbord 517

ASIA AND THE EAST

- NORIKO KAMACHI. *Reform in China: Huang Tsun-hsien and the Japanese Model*. By Daniel H. Bays 518
- MARY BROWN BULLOCK. *An American Transplant: The Rockefeller Foundation and Peking Union Medical College*. By Jessie G. Lutz 519

JOHN WHITNEY HALL <i>et al.</i> , eds. <i>Japan before Tokugawa: Political Consolidation and Economic Growth, 1500 to 1650</i> . By G. Cameron Hurst III	519	Corn As A Way of Life in Pioneer America. By John T. Schlebecker	536
ANN BOS RADWAN. <i>The Dutch in Western India, 1601–1632: A Study of Mutual Accommodation</i> . By M. N. Pearson	520	DAN SCHILLER. <i>Objectivity and the News: The Public and the Rise of Commercial Journalism</i> . By Donald W. Curl	537
PAUL HOCKINGS. <i>Ancient Hindu Refugees: Badaga Social History, 1550–1975</i> . By R. E. Frykenberg	521	THOMAS DUBLIN. <i>Women at Work: The Transformation of Work and Community in Lowell, Massachusetts, 1826–1860</i> . By John Borden Armstrong	538
STEPHEN FREDERIC DALE. <i>Islamic Society on the South Asian Frontier: The Mappilas of Malabar, 1498–1922</i> . By N. K. Wagle	521	BERNARD CRESAP. <i>Appomattox Commander: The Story of General E. O. C. Ord</i> . By Robert M. Utey	539
MARTIN J. MURRAY. <i>The Development of Capitalism in Colonial Indochina, 1870–1940</i> . By Truong Buu Lam	522	MICHAEL B. CHESSON. <i>Richmond After the War, 1865–1890</i> . By Leonard P. Curry	539
FRANK FARRELL. <i>International Socialism and Australian Labour: The Left in Australia, 1919–1939</i> . By Werner Levi	523	BURTON W. FOLSOM, JR. <i>Urban Capitalists: Entrepreneurs and City Growth in Pennsylvania's Lackawanna and Lehigh Regions, 1800–1920</i> . By Elisha P. Douglass	540
UNITED STATES		JUSTUS D. DOENECKE. <i>The Presidencies of James A. Garfield and Chester A. Arthur</i> . By Robert Marcus	541
JEROME M. CLUBB <i>et al.</i> <i>Partisan Realignment: Voters, Parties, and Government in American History</i> . By Allan J. Lichtman	523	RICHARD L. MCCORMICK. <i>From Realignment to Reform: Political Change in New York State, 1893–1910</i> . By Arthur Mann	541
WALTER NUGENT. <i>Structures of American Social History</i> . By Allan G. Bogue	524	LAWRENCE FOSTER. <i>Religion and Sexuality: Three American Communal Experiments of the Nineteenth Century</i> . By Carol Weisbrod	542
WILLIAM A. HAVILAND and MARJORY W. POWER. <i>The Original Vermonters: Native Inhabitants, Past and Present</i> . By Charles A. Bishop	525	HORACE L. FRIESS. <i>Felix Adler and Ethical Culture: Memories and Studies</i> . Ed. by FANNIA WEINGARTNER. By J. David Hoeveler, Jr.	542
MARY MAPLES DUNN <i>et al.</i> , eds. <i>The Papers of William Penn</i> . Vol. 1, 1644–1679. By Arthur J. Worrall	526	LAWRENCE D. ORTON. <i>Polish Detroit and the Kolasinski Affair</i> . By Edward R. Kantowicz	543
AUBREY C. LAND. <i>Colonial Maryland: A History</i> . By David W. Jordan	526	PATRICK J. GALLO. <i>Old Bread, New Wine: A Portrait of the Italian-Americans</i> . By John W. Briggs	544
WILLIAM E. NELSON. <i>Dispute and Conflict Resolution in Plymouth County, Massachusetts, 1725–1825</i> . By Hiller B. Zobel	527	LEO PAP. <i>The Portuguese-Americans</i> . By David M. Reimers	544
NORMAN FIERING. <i>Moral Philosophy at Seventeenth-Century Harvard: A Discipline in Transition</i> . By Edwin S. Gaustad	528	JOAN MARK. <i>Four Anthropologists: An American Science in Its Early Years</i> . By Fred Matthews	544
EDWARD M. GRIFFIN. <i>Old Brick: Charles Chauncy of Boston, 1705–1787</i> . By Robert Middlekauff	529	DAVID F. TRASK. <i>The War with Spain in 1898</i> . By Gerald F. Linderman	545
LESTER H. COHEN. <i>The Revolutionary Histories: Contemporary Narratives of the American Revolution</i> . By Charles Royster	529	DOMINICK CAVALLO. <i>Muscles and Morals: Organized Playgrounds and Urban Reform, 1880–1920</i> . By Louise C. Wade	546
SYLVIA R. FREY. <i>The British Soldier in America: A Social History of Military Life in the Revolutionary Period</i> . By W. Kent Hackmann	530	SARAH STAGE. <i>Female Complaints: Lydia Pinkham and the Business of Women's Medicine</i> . By Jane B. Donegan	547
F. DE BORJA MEDINA ROJAS. <i>José de Ezpeleta: Gobernador de la Mobila, 1780–1781</i> . By John J. Tepaske	531	NANCY SCHROM DYE. <i>As Equals and As Sisters: Feminism, the Labor Movement, and the Women's Trade Union League of New York</i> . By James Kenneally	547
RONALD HOFFMAN and PETER J. ALBERT, eds. <i>Diplomacy and Revolution: The Franco-American Alliance of 1778</i> . By Albert Hall Bowman	531	JUDITH ICKE ANDERSON. <i>William Howard Taft: An Intimate History</i> . By Paolo E. Coletta	548
GARRY WILLS. <i>Explaining America: The Federalist</i> . By Jacob E. Cooke	532	ROBERT E. FICKEN. <i>Lumber and Politics: The Career of Mark E. Reed</i> . By Kent D. Richards	549
ALLAN S. EVEREST. <i>The War of 1812 in the Champlain Valley</i> . By Reginald Horsman	533	LEONARD A. CARLSON. <i>Indians, Bureaucrats, and Land: The Dawes Act and the Decline of Indian Farming</i> . By Lawrence C. Kelly	550
ALEINE AUSTIN. <i>Matthew Lyon: "New Man" of the Democratic Revolution, 1749–1822</i> . By Rudolph M. Bell	534	PHILIP TAFT. <i>Organizing Dixie: Alabama Workers in the Industrial Era</i> . By Melvyn Dubofsky	550
DUMAS MALONE. <i>The Sage of Monticello</i> . By Noble E. Cunningham, Jr.	534	LEWIS A. ERENBERG. <i>Steppin' Out: New York Nightlife and the Transformation of American Culture, 1890–1930</i> . By John F. Kasson	551
J. LEITCH WRIGHT, JR. <i>The Only Land They Knew: The Tragic Story of the American Indians in the Old South</i> . By Mary Young	536	DAVID LEVERING LEWIS. <i>When Harlem Was in Vogue</i> . By Lawrence W. Levine	552
NICHOLAS P. HARDEMAN. <i>Shucks, Shocks, and Hominy Blocks:</i>		VICTOR GONDOS, JR. <i>J. Franklin Jameson and the Birth of the National Archives, 1906–1926</i> . By Nicholas C. Burckel	552

TOM D. CROUCH. <i>A Dream of Wings: Americans and the Airplane, 1875–1905</i> . By Alfred F. Hurley	553	TINSLEY E. YARBROUGH. <i>Judge Frank Johnson and Human Rights in Alabama</i> . By Robert B. Highsaw	564
ELEN C. SINGH. <i>The Spitsbergen (Svalbard) Question: United States Foreign Policy, 1907–1935</i> . By G. Bernhard Fedde	554	WILLIAM H. CHAFE. <i>Civilities and Civil Rights: Greensboro, North Carolina, and the Black Struggle for Freedom</i> . By Neil R. McMillen	565
PHILIP H. BURCH, JR. <i>Elites in American History</i> . Vol. 2, <i>The Civil War to the New Deal</i> . By John N. Ingham	554	PETER IVERSON. <i>The Navajo Nation</i> . By Gerald Thompson	566
SUSAN WARE. <i>Beyond Suffrage: Women in the New Deal</i> . By Lois W. Banner	555	ROBERT F. FUTREL and MARTIN BLUMENSON. <i>The Advisory Years to 1965</i> . By Russell H. Fifield	567
MARION CLAWSON. <i>New Deal Planning: The National Resources Planning Board</i> . By Ellis W. Hawley	556		
JOHN R. PETROCIK. <i>Party Coalitions: Realignment and the Decline of the New Deal Party System</i> . By Ralph M. Goldman	556	CANADA	
CHARLES W. JOHNSON and CHARLES O. JACKSON. <i>City Behind a Fence: Oak Ridge, Tennessee, 1942–1946</i> . By Richard G. Hewlett	557	CHARLES M. JOHNSTON. <i>McMaster University</i> . Vol. 2, <i>The Early Years in Hamilton, 1930–1957</i> ; STANLEY BRICE FROST. <i>McGill University: For the Advancement of Learning</i> , Vol. 1, 1801–1895. By Karen C. Altfest	567
JAMES H. JONES. <i>Bad Blood: The Tuskegee Syphilis Experiment</i> . By Kenneth F. Kiple	558		
C. A. MACDONALD. <i>The United States, Britain, and Appeasement, 1936–1939</i> ; TERRY H. ANDERSON. <i>The United States, Great Britain, and the Cold War, 1944–1947</i> . By Robert A. Divine	558	LATIN AMERICA	
RUSSELL F. WEIGLEY. <i>Eisenhower's Lieutenants: The Campaign of France and Germany, 1944–1945</i> . By Forrest C. Pogue	559	ARTHUR MACEWAN. <i>Revolution and Economic Development in Cuba</i> . By Jorge I. Dominquez	568
ROBERT H. FERRELL, ed. <i>The Autobiography of Harry S. Truman</i> ; ROBERT H. FERRELL, ed. <i>The Eisenhower Diaries</i> ; ROBERT H. FERRELL, ed. <i>Off the Record: The Private Papers of Harry S. Truman</i> . By Barry D. Karl	560	PAUL J. VANDERWOOD. <i>Disorder and Progress: Bandits, Police, and Mexican Development</i> . By Don M. Coerver	569
BLANCHE WIESEN COOK. <i>The Declassified Eisenhower: A Divided Legacy</i> . By Charles C. Alexander	561	PETER V. N. HENDERSON. <i>Félix Díaz, the Porfirians, and the Mexican Revolution</i> . By Ramón Eduardo Ruiz	570
MICHAEL BALFOUR. <i>The Adversaries: America, Russia, and the Open World, 1941–62</i> . By John Lewis Gaddis	562	CYNTHIA MCCLINTOCK. <i>Peasant Cooperatives and Political Change in Peru</i> . By Thomas M. Davies, Jr.	570
PAUL B. RYAN. <i>First Line of Defense: The U.S. Navy since 1945</i> . By Benjamin Franklin Cooling	563	MICHAEL GROW. <i>The Good Neighbor Policy and Authoritarianism in Paraguay: United States Economic Expansion and Great-Power Rivalry in Latin America during World War II</i> . By Ronald C. Newton	571
THOMAS R. DUNLAP. <i>DDT: Scientists, Citizens, and Public Policy</i> . By Morgan Sherwood	564	STANLEY E. HILTON. <i>Hitler's Secret War in South America, 1939–1945: German Military Espionage and Allied Counter-espionage in Brazil</i> . By David Kahn	572
		JOHN W. F. DULLES. <i>President Castello Branco: Brazilian Reformer</i> . By Jordan M. Young	573
Collected Essays	574	Communications	591
Documents and Bibliographies	583	Index of Advertisers	34a
Other Books Received	586		

The Limits and Divisions of British History: In Search of the Unknown Subject

J. G. A. POCKOCK

THIS ESSAY CONTINUES A PROJECT begun some time ago—that of defining a field of study that might properly be termed “British history” and providing a conceptual framework within which such history might be written.¹ This project, however, involves more than a mere revision of parameters and entails some reflections on the social character and purposes of historiography itself. The reasons why British history has not been written are good, in the sense that they follow naturally enough from the adoption of a limited, if perfectly legitimate, conception of the historian’s function. If we want a British history at all, we must detach ourselves from this conception, circumscribing though not necessarily denying its legitimacy, and provide ourselves with another—perhaps limited in different ways—that will legitimate the history we want to write. It may turn out, however, that we want a British history because we have, for reasons yet unstated, begun to detach ourselves from the conception of historiography that has impeded its being written and to adopt another that calls for a “British history”—whatever that may turn out to mean.

It is a fundamental premise of this project that we have little or no “British history,” in the sense of the history of an entity to which the term “Britain” is regularly and intelligibly applied; most of what passes by that name is English history and makes little pretense of being anything else. The average textbook with “British history” in its title is overwhelmingly, and in a real sense exclusively, concerned with the society and politics of the realm and culture known as “England”; the history of other “British” realms and cultures is presented minimal-

¹ There are three previous stages to this project: (1) “British History: A Plea for a New Subject,” *New Zealand Historical Journal*, 8 (1974): 3–21, reprinted in the *Journal of Modern History*, 4 (1975): 601–24, with comments by A. J. P. Taylor, Michael Hechter, and Gordon Donaldson, and a reply by the author, *ibid.*, 625–29; (2) “The Limits and Divisions of British History,” paper delivered to the Modern European History Section of the American Historical Association at the Ninety-Third Annual Meeting of the American Historical Association, held in San Francisco, December 28–30, 1978, and distributed as *Studies in Public Policy*, no. 21, by the Center for the Study of Public Policy, University of Strathclyde (Glasgow, 1979); and (3) a working paper written for a projected (but now suspended) multivolume history of early modern Britain. I wish to thank the following for comment on various drafts: Rowland T. Berthoff, Washington University, St. Louis; J. H. Grainger, formerly of the Australian National University; Geoffrey Parker, University of St. Andrews; Richard Rose, University of Strathclyde; Richard S. Tompson, University of Utah; Stephen Saunders Webb, Syracuse University; Arthur H. Williamson, New York University; and G. W. O. Woodward, University of Canterbury.

ly—and very often in terms that suggest that it is irrelevant where it is not intrusive. Conversely, there are many (perhaps more) “histories of England” and works self-described as studies of “English history,” written on the premise—usually but not always unstated—that “English history” does not form part of a larger subject called “British history” and does not need to be written with reference to the latter. When such a series as the *Oxford History of England* (1934–65) or the *New History of England* (1978–) arrives at the years 1707 or 1801, the momentous Acts of Union marked by these dates are of course given thorough and proper treatment; but there is no caesura, no change of key or structure, no sense that the history of England has become part of something else and requires to be written in new terms.² It simply continues, modified in so far as the association with Scotland or Ireland is seen to have modified it; and, if a union so presented is hardly relevant to the continuity of English history, it is not to be expected that any attempt will be made to depict it as continuing an already living Scottish or Irish history. That perspective is left to the authors of histories of Scotland or Ireland, who write as if they were addressing themselves to different reading publics, so that once again it is clearly implied that there is no such thing as “British history,” no entity called “Britain” whose history should or can be written. Yet it is also implied by the most simply stated of historicist premises that an entity without a history does not exist in history—a somewhat ominous implication in an age when the unity and sovereignty of “the United Kingdom” are exposed to various challenges.

There is a perspective in which it is possible to justify this way of writing British or English history and show that there is a *verità effettuale* that it does not distort; and to explore this perspective may tell us much about the character of both “British history” and modern historiography. The *verità effettuale* in question is the autonomy of English politics, which is affirmed to explain its own history whether or not it forms part of the politics of a “Great Britain,” a “United Kingdom,” an Empire, or a Commonwealth. In the case of 1707, the structure of parliamentary politics and the foundations of this structure in English social relationships may be said, with a good deal of cogency, not to have been substantially affected by the parliamentary union with Scotland, so that their history after 1707 may justifiably be written within the same parameters as those used before it. It follows that the relations between parliamentary politics and social structure in Scotland are to be studied (if at all) by a separate group of historians, in the same way that the Scottish Reform Bill of 1832 had to be written by a separate group of draftsmen inhabiting a separate political culture (although whether the history of the popular agitations preceding 1832 in England and Scotland can be written as two unconnected stories is another matter). But the rattle of loaded dice is to be heard as soon as we resort to *verità effettuale*.

² See G. N. Clark, *The Later Stuarts, 1660–1714*, volume 10 of the *Oxford History of England* (Oxford, 1934), chap. 10: “Relations with Scotland,” particularly 281: “. . . it is not meaningless to ask whether it [the Union of 1707] was the basis only of the later development of the life of the state or whether its results extended into social life, into literature and learning and individual character.” Also see J. Steven Watson, *The Reign of George III, 1760–1815*, volume 12 of the *Oxford History of England* (Oxford, 1960), 387–405; and J. R. Jones, *Country and Court: England, 1685–1714*, volume 5 of the *New History of England* (Cambridge, Mass., 1978), 329–33.

Because the effective determinants of power lay in England, the history of Anglo-Scottish union is English history—not because the relations of power to society in Scotland were assimilated to those in England but because they were excluded and could be largely ignored. English historians of the period have, therefore, no need to study Scottish history, and Scottish historians interested in *verità effettuale* might do better to study English history than Scottish. This indeed seems to have been the decision of the great Scottish historians from Hume to Macaulay,³ who with one accord aimed at an understanding of English (and European) history more sophisticated than any the English could construct for themselves but who increasingly relegated Scottish history to the status of “an auld sang” or “the tales of a grandfather.” To write the history of Britain, viewed as the interaction of several peoples and several histories, does not seem to have struck them as *effettuale*; nor did they continue to write Scottish history as a series of social and political processes, still taking place within “Britain” and forming part of its history.⁴

There is, of course, a Scottish nationalist historiography, written out of opposition to the Union, which represents Scottish history as a *verità quasi-effettuale*: the history of a people intelligible within the parameters they have constructed for themselves but overlaid, repressed, and distorted by the imposition of a “British” structure, which is English and irrelevant. Such historiographies have been still more effectually developed in those cultures—the American and the Irish, for example—that have succeeded in becoming independent of that English-dominated union or empire to which alone the word “Britain” seems at this point to be applicable. “Irish history” is effectually asserted to have been an intelligible reality since pre-Christian times; “American history” since the Revolutionary transformation if not since the original European settlements. But nationalist historiography is in an important sense a double-edged weapon. Because English governing institutions were autonomous to the point where they could exercise empire over Scots within the parliamentary union and Americans and Irish outside it, their history could be written as if strangers had played no part in it. Scots responded by joining in writing it on these presumptions. But, when empire failed in America or union in Ireland, and Americans and Irish presaged their departure by writing nationalist historiography in which the English were the strangers (John Adams and Thomas Jefferson were doing this by 1776⁵), they implicitly assured the English of what the

³ Despite his name, Macaulay was thoroughly Anglicized, and his great work, though a thorough study of all three kingdoms, is nevertheless a *History of England*. In his interpretation of history, however, he owed more than he might have wished to Hume, and, in the organization of his research, a great deal to Sir James Mackintosh. We have as yet no full-length study of the growth of Whig historiography from its English and Scottish sources.

⁴ A possible exception is Robert Henry, *The History of Great Britain, from the First Invasion of It by the Romans under Julius Caesar, Written on a New Plan*, 6 vols. (London, 1771–93). William Robertson’s *History of Scotland* was his first work, published in 1759; it concludes with the observation that the Union of the Crowns in 1603 terminated the independent development of the Scottish literary language, whereas the Union of the Parliaments in 1707 made possible the complete assimilation of the Scottish forms with English; Robertson, *The Works of William Robertson*, 2 (London, 1824): 243–48.

⁵ Adams’s *Letters of Novanglus* (1775) and Jefferson’s *Rights of British America* (1775) uphold the thesis that the colonists emigrated from the English king’s dominions and possessed thereafter only such relations with the Crown as they formed by contract.

latter already took for granted: their institutions and history were independent of the empire they had acquired and were about to lose, and there was no need to fear the fate of Rome, which had been absorbed by its own empire and had disintegrated with it. An Anglocentric and an Anglophobic historiography, therefore, form two sides of the same medal; they reveal the former as ideological means both of maintaining empire and of surviving its loss. Only in our own times has the simultaneous loss of maritime supremacy and of a historic relation to Europe afflicted the English consciousness with something like a crisis of identity, and it would be interesting to consider how the writing of history is reacting to it. In the meantime, it must be admitted that a historiography that presupposes the uniqueness and intelligibility of England will not be transformed into a historiography of Britain by the simple act of adding to it an independently constructed nationalist historiography of Scotland or Ireland; the motives that lead to the creation of the latter do not require that a "history of Britain" should be written at all. Once again, however, historicist premises must make us ask whether "Britain" is either rational or real.

BUT THERE IS MORE TO HISTORIOGRAPHY than ideology; a *verità effettuale* is, almost by definition, historically intelligible. The unifying theme of English historiography, from the Ancient Constitution through the "Whig interpretation" to its twentieth-century successors,⁶ has been the interaction of governing institutions with a continuous social fabric; and this has been so because English society has been continuously and effectively governed by those institutions, to the point where the Union of 1707 and even that of 1801 made little difference to their functioning or to what made it effective. In this perspective, the Unions themselves occurred as the product of external rather than internal necessities; Scotland and Ireland could not be ignored, but England could function well enough without them. But to say so much is to admit the truth of the contention that England is historically intelligible within the parameters on which English historians have chosen to focus their attention: that English history can be satisfactorily written—as it has been—while British history is ignored or relegated to the margins of reality. We have not yet discovered a perspective in which "British history" is a *verità effettuale*: a reality that determines the present and renders a past intelligible.

We have so far been using a set of assumptions about the nature and purpose of historiography that are generally thought of as belonging to the later nineteenth and earlier twentieth centuries. In this vision it is presumed that history is past politics: historiography originates as the memory of the state and develops as the critical study of the processes that have brought the state into being. The historian devotes attention to the activities characteristic of the state: war and diplomacy, administration and politics, the participation of society in its own government; even

⁶ See P. B. M. Blaas, *Continuity and Anachronism: Parliamentary and Constitutional Development in Whig Historiography and in the Anti-Whig Reaction between 1890 and 1930* (The Hague, 1978).

the historian's concern with social structure is not impeded, but is organized, by an overmastering concern with its politicization. Where states exist, they can organize their pasts in this way; where states have been the product of historical forces operating over long periods of time, those periods seem predisposed to be organized into history of this familiar modern-classical kind. When the desire arises to write history upon premises to which the state is not central, the "new history" will be in tension with history that continues to be written along the older lines.

The situation changes when the "state" is not something that has been in formation for centuries but a recent, artificial, or revolutionary creation. Here we encounter the problem of the "nation-state" as it presented itself to modern European minds. It was one thing to write the history of "France" when that entity seemed to have been created by a monarchy spreading out over the centuries from its base in the Île de France and quite another to write that of "Italy" when the conjunction of Piedmontese monarchy with a variety of local elites had occurred unexpectedly, violently, and most inorganically within a few recent years. In the one case it might be said that *l'état, c'est l'histoire*; in the other, *regno* and *stato* had possessed no *storia* before the two had to be created together. But, although it has been easy to point out that "Italy" in 1871 had little common language, culture, or consciousness and that the writing of "Italian history" has been a (rather daunting) way of making good these deficiencies, it has not been quite possible to ignore that the same was true in lesser degree of France. We now possess a number of studies of French historiography as intended not merely to legitimate the Third Republic but to create a consciousness of national unity based upon that legitimation;⁷ and in both national cultures we find tensions between *storia statale* and *storia civile*, *histoire événementielle* and *histoire totale*, the historiography of the nation-state and the historiography of society and material culture viewed independently of their politicization.

It is clearly possible to enlarge this account into a model for the predicaments of modern historiography, and use it to inquire how "orthodox" state-centered historiography is to co-exist—if at all—with history of the newer kinds. But, before doing so in the present context, we must take account of certain difficulties that arise as soon as we attempt to apply the nation-state model to either the *verità effettuale* of English history or the problem of British history. In the first place, English history has indeed been dominated by the past of a political structure, which may be said to have shaped the national history and provided the parameters within which it is intelligible; but it is not at all easy to describe that structure as a "state," even in comparatively modern times. The governing paradigms of Whig historiography—which nothing seems to invalidate—insist on presenting it as a symbiosis of government and society, of ruling elites with their frequently turbulent clients and dependents; and terms like "the English" or "the British state" are often

⁷ Eugen Weber, *Peasants into Frenchmen: The Modernization of Rural France, 1870–1914* (Stanford, 1976); William R. Keylor, *Academy and Community: The Foundation of the French Historical Profession* (Cambridge, Mass., 1975); and Charles-Olivier Carbonnel, *Histoire et historiens: Une Mutation idéologique des historiens français, 1865–1885* (Toulouse, 1976).

employed by scholars who wish (rightly) to point out that this is a model of class dominance and think (less satisfactorily) that calling it a "state" is an effective way of saying so.⁸ In the second place, it is far from clear that what this structure rules is a nation-state and quite certain that it does not cause the history of one to be written. The English ruling structure—let the word "establishment" drop exhausted into the dustbin of history—may have organized a geographically defined culture into a nation and in some sort a state; the history of how it has done so is not free from blind spots but constitutes one of the great historiographical enterprises of the world. It may have assimilated the Welsh and Scottish cultures to the point where the continued unity of the state so formed may be expected to survive its challenges; but a historiography of how (or whether) this has been achieved is so far lacking that we are unable to define "British history" at all. If the "Kingdom of Great Britain" formed by incorporating Scotland into the fusion of England and Wales between 1707 and 1801 was not a nation-state and did not possess the historiography of one, the "United Kingdom of Great Britain and Ireland" (1801–1922) and its successor, the "United Kingdom of Great Britain and Northern Ireland" (1922–), were exposed to constant challenges to their unity as states; and their historiography may be thought of as flowing in two channels, one English, the other Irish—neither British. The invincible persistence of English historiography in the midst of this shifting pattern of impermanent state forms may be explained in two ways: the high stability of English social and political structures, and the ideological capability of Anglocentric historiography to double as an instrument of empire and of withdrawal from empire. But it is a consequence of both that the model of nation-state historiography does not fit, or fits only paradoxically (if it fits as paradoxically in the case of Italy, Germany, or even France, the reasons are different). A historiography that either intends or achieves the consolidation of union, empire, or commonwealth is not really to be found. The *verità effettuale* needs a history in only some of its capacities.

If the nation-state model produces some unexpected results when applied to "Britain," it is nevertheless true that "England" possesses a historiography of the modern-classical kind, centered on the processes of formation of a dominant political structure. This is open to the objections usually brought against historiography of the kind, and it is possible and legitimate to envisage the growth of an alternative historiography focused on demography, social structure, community, material culture, and the like. There is no reason, so far as the present analysis goes, why something in the nature of a British *Annales* school should not take shape, or why the complex geography of the major islands should not furnish a British or Irish Braudel with his *monde méditerranéen*. Here indeed is one way in which a "British history" might come into being and be written; for, as far as we know at this stage of our inquiry, an Anglocentric "Whig" historiography is never going to

⁸ For an exception to this rule, see J. R. Western, *Monarchy and Revolution: The English State in the 1680s* (London, 1972). Western seems to have employed the term in order to emphasize how close England came to the establishment of an absolute monarchy. It would be valuable to have a history of the word "state" in English political and historical literature.

produce one or offer any reasons why one should be constructed. We should indeed pause at this stage to ask on what grounds we are proposing that there ought to be a "British history" and that the inquiry is worth conducting.

The premises must be that the various peoples and nations, ethnic cultures, social structures, and locally defined communities, which have from time to time existed in the area known as "Great Britain and Ireland," have not only acted so as to create the conditions of their several existences but have also interacted so as to modify the conditions of one another's existence and that there are processes here whose history can and should be studied. The extent to which these processes have formed a self-contained pattern should not be exaggerated; there have been interactions with adjacent Continental Europe—Languedoc, Normandy, the Netherlands, Scandinavia—that make their own claims to attention. But a history that takes place in an insular situation can for more than merely verbal reasons be studied in a degree of isolation. "British history" in various ways takes place on the geographical and cultural frontiers of "Europe," and, if we insist—as it is now the fashion to do—that it is and has always been part of "European history," we are giving a certain meaning to the latter term as well as to the former. There have been Celtic, archipelagic, American, and oceanic regions of social and historic experience with which what we call "England" has interacted; the term "British history" may be used to isolate this series of interactions and relate it to the history of "Europe" in whatever way seems fitting.

If the premise that the societies and cultures of the large islands lying off northwestern Europe have interacted and share a history is found plausible, the suggestion that their interactions have occurred and can be studied on many levels will also seem plausible. We have envisaged the writing of a "British history" along "Braudelian" lines, which emphasize the historical geography of economy, society, and culture; and it would be exciting to explore this possibility further. But it is notorious—although it does not seem to be necessary—that in such a perspective the role of the political becomes problematic. There is a tendency to relegate politics to the surface realm of mere events, perhaps because political structures bring to consciousness a public space in which events are seen to take place and possess significance, and narrative historiography has been overwhelmingly the record of such events; and *histoire totale* seems to be enduringly at tension with that historiography, ancient and modern, which is the product of the politicized consciousness. The present essay, as must already be evident, is written on the premise that this tension is likely to continue. To depoliticize the consciousness of Westernized historical mankind seems about as unpromising an enterprise as can be imagined, and there is indeed no school of historians known to have undertaken it. As always, it is excellent that there should be some systematic challenge to the view that history is past politics, but—if only because politicization is a major engine in the transformation of human consciousness—it appears both reasonable and valuable that there should continue to be historiography devoted to the processes of formation of political structures, states, and societies. After acknowledging and saluting the possibility of a Braudelian "British history," therefore, this essay will

pursue the enterprise of asking how such a history might be constructed along lines that become salient when priority is given to political formation.

AMONG THE FIRST PROBLEMS that this enterprise confronts must be that of geopolitical nomenclature. Whatever "British history" may be, it has not been confined to the island that the cartographers have named "Britain." The processes of politicization, of the formation and disruption of state structures, which the term denotes and includes, have extended themselves, in the first place, to the large adjacent island known as "Ireland"; and, because there has been an Irish nationalist reaction, leading to the creation of an independent state with its own historiography, the notion that "Irish history" forms part of "British history," or even part of the history of "the British Isles," is often denied. Should the term "British Isles" be rejected on political and nationalistic grounds, then for the same reasons we have no agreed nomenclature, even on the simply cartographic level, and must try instead to familiarize ourselves with some new and neutral term: perhaps "the Atlantic archipelago," denoting everything from Shetland to Sark and from Dingle Bay to the Dogger Bank. But the term "British history" has been used here to express the need for something that shall include both the attempt to incorporate Ireland within English or British political structures, and the reactions against that attempt and its consequences. The history of Irish nationality is as much a part of "British history" in this sense as is the history of Union and Empire, and "British history" thus denotes the historiography of no single nation but of a problematic and uncompleted experiment in the creation and interaction of several nations. At this point, "British history" becomes independent of the fate of Anglo-Irish union and will continue as long as there are a number of nations inhabiting "the Atlantic archipelago."

But "British history" does not stop there; it extends itself into oceanic, American, and global dimensions. In the seventeenth and eighteenth centuries, the Atlantic seaboard of North America became incorporated in "English history" and acquired inhabitants with modes of consciousness corresponding to this experience. The revolutionary transformation whereby a sector of this area became politically independent and developed a consciousness and historiography of itself as "America" must of course be considered as the emergence and development of "American history"; but it must equally be considered as the transformation of an English or British subculture in the late Whig world, not to be understood without comprehension of the "American" trauma. Our "British history" therefore includes the history of English-speaking America down to the point—however we determine it—when the United States is seen to have created a distinctive political culture and embarked upon a continental and global history that demands to be treated in its own terms. The pattern resembles that of Irish history in the sense that Anglicization and the revolt against Anglicization unite in determining it; "Ireland," however, is not able to depart from that archipelagic history of which "America" ceased to be an extension.

There continues after 1783 to exist a British North America, subsequently

known as Canada, whose history may be considered in two ways. We may regard it as a continuation of Anglo-American history on terms differing from those which the United States has laid down for itself; we may also consider it in the context of that stream of emigration from nineteenth-century Britain and Ireland, which—though flowing for the main part into the United States—created at the same time a number of colonies of settlement, merging to form the “dominions” of the old “British Commonwealth.” At this point “British history,” both cultural and political, is discovered to have exceeded even the archipelagic and Atlantic dimension and to have established itself in a number of areas in the southern hemisphere. The history of these settler nations—at least those of them that survive⁹—makes a claim to be considered part of “British history” and to enlarge the meaning of that term. Being the history of British (and Irish) culture under politically autonomous conditions, it cannot—least of all when it takes a nationalist turn—be considered along with the history of Asian and African societies in the rapidly exploding and collapsing category of “imperial” or “colonial” history.¹⁰ This decision obliges us to conceive of “British history” no longer as being an archipelagic or even an Atlantic-American phenomenon, but as having occurred on a planetary scale; and it is resistant to that current insistence that “British history” is a subdepartment of the “history of Europe,” which is so plainly an ideological response to the disintegration of power on the planetary scale. The author of this essay offers himself as historical evidence that it is possible to be of English–South African and Anglo-Norman–Channel Island descent, and of New Zealand cultural formation, and so to know oneself to be British but neither English, Irish, American, nor European. It may be added that possession of such a consciousness is one very good reason for perceiving as necessary such a “British history” as is outlined below. At every moment in its growth, this “British history” may be perceived as both imperial and postimperial.

WHAT ARE THE POLITICAL CATEGORIES in which the projected history might be written? Since the political system thematically dominant in the whole story is English and possesses a historiography in which British history is largely ignored or, at best, treated as an external reality, new categories are needed. The historiography of English government in its relation to English society is not precisely the historiography of a state. Not only is the word “state” somehow inappropriate to a political system historically dominated by magistrates and representatives rather than administrators,¹¹ but its historiography is not fully that of the structural developments by which “England” has become “Britain” and “the United King-

⁹ I am presuming that the English-speaking communities of Kenya, Rhodesia-Zimbabwe, and South Africa are in process of absorption by African or Afrikaner nationalism.

¹⁰ This category—in which “Commonwealth history” may be included—has a place as the history of imperial administration, policy, and association, without including much internal history of the societies concerned. In the New Zealand university curriculum, for example, “New Zealand history” is one thing and “Commonwealth history” another. It is worth mentioning that “Australian history” used to have very little place in that curriculum and that this neglect was thought to be reciprocated.

¹¹ It can of course be contended that the reaction against “Whig historiography” was occasioned by an increasing concern with the administrative dimension; see Blaas, *Continuity and Anachronism*.

dom.” Even if these are “states” (and it is not clear that we should call them that), they were not created by the simple assimilation of new territories to an English state structure. The Statute of Wales (1536), it is true, completed the organization of Welsh lands into English shires, and so the assimilation of Wales to the typically English governing structure of county, borough, common law, and Parliament. But the Unions of 1707 and 1801 were legislative unions, formed by the incorporation with the English parliament of Scottish and Irish parliaments, whose relations with shires and boroughs—to say nothing of established churches—differed from the English pattern and in many ways survived the Union. Once again, it is doubtful whether what was formed can be called a state,¹² since it is not fully unified and is not federated either, and we may have to endorse the judgment of those political scientists who classify “Britain” among “stateless societies” or societies lacking a “state tradition.”¹³

If the concept of “nation-state” is failing us at all points, however, there remains the hard fact of Union: there has been created an effective sovereignty by which a crown in parliament legislates for a “Britain” larger than “England.” This alone calls for a historiography that cannot be confined to the history of England. But such a historiography, which does not yet exist and must be created, cannot be written as the memory of a single state or nation or as the process by which one came into existence. It must be a plural history, tracing the processes by which a diversity of societies, nationalities, and political structures came into being and situating in the history of each and in the history of their interactions the processes that have led them to whatever forms of association or unity exist in the present or have existed in the past. This calls for a multi-contextual history, and such are notoriously difficult to write. They become more difficult still when the constellation of social and political structures is seen to expand beyond the seas, creating as it goes new contexts in which its history must be interpreted or reinterpreted.

It is ideally desirable to return to a point in time before any of the structures formed in subsequent history could be said to exist. In British history this might mean adopting an “Arthurian” perspective¹⁴ and looking at the state of the archipelago at the time of the disintegration of the Roman province, which had been formed in the larger part of the larger island. Because the Romans did not dominate the navigation of the Irish Sea or the North Sea to the point where they could control the Irish or German-Scandinavian land masses that lay beyond them, their empire had here a maritime *limes*; and we might generalize that archipelagic history has lain both on and beyond the frontiers of Roman and subsequently European history. The estuaries looking toward Europe have given access to various forms of power and culture developed on the Continent, which have established provinces and kingdoms in the valleys and lowlands; and these have competed for power with structures formed on the adjacent European mainland.

¹² See Richard Rose, “The United Kingdom as an Intellectual Puzzle,” *Studies in Public Policy*, no. 7 (Glasgow, 1977).

¹³ Kenneth Dyson, *The State Tradition in Western Europe* (New York, 1980).

¹⁴ See John Morris, *The Age of Arthur: A History of the British Isles from 350 to 650* (London, 1973).

At the same time, it has been a question how far they have extended their control of upland and maritime regions farther north, west, and beyond seas, which have had their own pelagic contacts with each other and with larger universes.¹⁵

The “Arthurian” and “paleo-English” periods, spanning perhaps five centuries, include a good deal of movement and formation of societies—Roman-British, English, Gaelic, and Scandinavian—both within the archipelago and across the seas, which the Romans did not dominate. Because there is movement between the major islands, we are justified in looking at the archipelago as a whole, which is not to say that its history is self-contained; contacts with both post-Roman and non-Roman Europe are to be discerned. A map takes shape on which the peoples who later constituted “England,” “Ireland,” and “Scotland” become visible, although these geographical generalities are not yet the names of political or perhaps even historical realities. There is no period of “British history” about which the literary organization of historical memory has less to tell us in comparison with the techniques of archaeology and linguistics; and a history of the archipelago constructed in terms of its *durées* will lay its foundations here, unencumbered by the subjectivities of memory. But a diversity of systems of domination and adjudication, which we may choose to term “political,” can be seen taking shape among the peoples in formation, and their growth is advanced by the establishment of literacy. We note the growth of ecclesiastical structure and clerical culture, of royal government by means of writ, warrant, and books of law. As the clerks take hold of administration, their products include historiography, whether this records the ascendancy of the monastery, the king, or the heroic lineage. This historiography inscribes the continuity of governing structures in human consciousness and teaches men and women to identify themselves as members of the systems that rule and mobilize them; nations are created by states, and historiography is both the instrument and the record of this process. As the political structures whose history we aim to write take shape, so do the schemata by which our historical consciousness is organized. It is the problem of “British history” that this activity has not been uniformly distributed.

What may be termed the “medieval” period of this history is based, first, upon the formation of an English kingdom of fairly stable character in Wessex, Mercia, and East Anglia, with associated earldoms to the north—the area so circumscribed becoming known as “England”—and, second, upon its conquest and modification by Normans in the eleventh century. The Norman role in British history is enlarged by the activities of military adventurers beyond historic “England.” They are found where the uplands of Northumbria, not yet fully incorporated in either realm, merge into that lowland kingdom from which historic “Scotland” is derived; and they sharply modify that kingdom’s interactions with Picts, Scots, and Norse. They appear, building castles and appropriating lands, among the British principalities of Wales and, in a moment heavy with consequences, across the central sea

¹⁵ It might be argued that this presentation does too little to account for the Scandinavian presence in the archipelago.

of the archipelago in Ireland. Here, as all have reason to know, the Anglo-Norman kingship follows them, asserting a suzerainty it does not wish to see them escape; the central theme of Anglo-Irish history appears to have begun and provides a moment to reflect on the character of empire—especially medieval empire—as it takes shape on the frontiers of settled government. Here it is economical to suggest a model.

Beyond the zone of law defined by settled government, there is a zone of war, in which the king or his subjects can make their presence effective only in arms. They are more conscious of the actions of war in this zone than of other activities and relationships; but the “zone of war” may not seem so to its own inhabitants. Local economic and military activities—carried on by both settlers and natives borrowing techniques from one another—lead to the formation of power structures beyond the king’s immediate reach; and, especially if these are the work of those otherwise his subjects, he has to decide whether to attempt to control them, to leave them to themselves, or to attempt indirect control or some other middle course. But in proportion as the king rules his immediate domain as a zone of law rather than war—through the management of social relations by writs and documents rather than the imposition of command by the sword—there comes into being a world of stable social relationships in which the king is himself involved and which makes demands upon him while increasing his resources. He may rule a bifurcated realm, or a realm having two faces: it is theoretically possible to distinguish between the “domain,” where his writ runs and his clerks of justice assert his sedentary authority, and the “march,” where his power is the sum of his relations with powerful military figures, feudatories or tributaries, subjects or aliens.

Very complex cultural and social systems can grow up in the “march,” which is typically a zone of acculturation; peasant soldiers learn nomad warfare, Normans become more Irish than the Irish themselves.¹⁶ Under feudal conditions, where the rule of law is hard to distinguish from the rule of the sword, the “domain” and the “march” may be hard to tell apart; and, under a wide variety of conditions, the politics of the latter may intervene in the politics of the former: the border chiefs may clear the king’s causeway, the armies of the frontier may march on the imperial palace. The inhabitants of the “domain” know this and may make their own demands on the king, calling on him to extend or retract his attempts to control the “march,” assuring them greater security at less cost. Their behaviour is part of their consciousness of an increasingly dense and complex world of social relationships, which the king’s law has made possible and apparent to their understandings. The king is involved in these relationships, and his involvement makes it possible for them to bring pressures to bear on him; in the terms of the model, “polity” is the appropriate term for the political world of the domain and its settled society. Since literacy is presumed to be peculiar to the domain and a principal instrument in its creation, one consequence among others will be that history comes to be written in and for the domain, and will be the history of the polity: of a world of legally

¹⁶ For the former illustration I am indebted to Owen Lattimore, *The Inner Asian Frontiers of China* (New York, 1940); for the latter, see Robin Frame, “Power and Society in the Lordship of Ireland, 1272–1377,” *Past & Present*, no. 76 (1977): 3–33.

determined relationships, to which the politics and history of the march appear external.

This is an essentially simple model, which proves to be far from adequate to describe the complex realities of any concrete situation. It may be useful, however, in clarifying the outlines of British history and the growth of its historiography. The Normanized kings of the English and Scottish lines appear to have been figures in a process of military settlement and expansion coherent enough to create the problems of empire. As far afield as Ireland, military adventurers set up forms of power, interacting across cultural barriers with those which native leaders set up to counter them. Both in turn interacted with the forms of power set up by the pursuing kings in their attempts to exert direct or indirect control over these areas of enterprise and reaction: marcher lordships, tributary chieftaincies, garrisons, colonies. New modes of domination and mobilization were created, and in some cases powerfully affected the social organization and cultural identity of the peoples concerned; a wide variety of these came into being in western and northern areas of the archipelago, rendered more complex still by the interventions of churchmen in the process. A great deal of "British history" must be located in these highlands and maritime regions; we move from "Europe" into the "Atlantic." But, especially in the English case, the kings ruled a southern and eastern lowland of agriculture, literacy, and law, which provided some of the resources necessary for expansion into the pasture and pisciculture of the highlands and islands, but presented them with challenges and problems that assumed political priority. Here there took shape that "polity"—that interaction of government and society—around which the English nation may have taken shape and which certainly provided it with the dominant themes of its historiography. By the thirteenth century it was possible to speak of a *communitas regni*, which profited by the king's peace but for that very reason was able to object strenuously and militantly to some of the king's activities; the English realm was hard to govern precisely because it was intensively governed. Some, though by no means all, of the problems contested arose from the kings' engagement in the pursuit or avoidance of empire in Britain and the archipelago; and, to the degree to which this was so, the expansion of empire and the opposition to empire are both effects of the formation of a polity of settled government within a military system tending to outgrow it. The community of the realm, like the king himself, had an uncertain relationship to the marcher lordships and the world they inhabited.

This may, indeed, be the secret of a historiography that serves both empire and the opposition to empire by consistently treating the affairs of the polity as the central, and those of the empire as the marginal, reality. This historiography does so because it is the polity's creation, expressing both the polity's ambiguities on the problem of empire and its determination to control its own affairs and guard them against being determined by attempts to solve that problem. But the English is not the only polity to have come into being in the course of British history. There is the case of Scotland; and marches may evolve into settled polities and begin to create their own historiographies. Only the march itself, the moving frontier of military expansion and reaction, never writes its own history.

WE SHOULD NOT THINK OF THE MEDIEVAL KINGS as perpetually or primarily involved in the problems of empire within Britain and the archipelago. Edward I and Edward II may be seen as engaged in an attempt to bring the feudal lordships of Wales and Scotland permanently under their jurisdiction, succeeding in the one case and failing in the other, while the Norman-Irish lordships remained so far beyond the medieval reach as to be exposed only to attempts at indirect control. But attention has to be paid to the involvement of Angevin, Plantagenet, and Lancastrian kings in the affairs of Normandy and Aquitaine, extending at times to attempts at controlling the French crown itself; and, as the most permanent involvement of medieval Britain in adjacent Continental Europe, these great episodes present a problem in the conceptualization of British history. Do they show us the medieval kingdoms as essentially European and committed to Europe? Or are they essentially trivial and destructive, an external and menacing disturbance in the self-generated growth of the English and French royal polities? To answer such questions we should have to determine whether the growth of English institutions of law and government—the perdurable theme of English medievalist historiography—was unaffected or only externally affected by institutional growth in Bordeaux or Gascony, or whether the same developments can be seen taking lasting root in the insular and Continental realms of the Angevin empire. Here, as in the inquiry into British history, we should be testing the fundamental hypothesis on which all English historiography is built: the premise that the relationship of crown to shire, like the common law and Parliament that grow from it, is unique, the innermost secret of English history, the root from which everything proceeds and to which everything else is only externally relevant. This premise may very well be true; its present standing among medieval historians will not be considered here. Since the Continental realms of the Angevin-Plantagenet empire were ultimately absorbed into France, however, their history may have been written, by French scholars of the old tradition, on the premise that only French categories could explain it; and this may have been reinforced by the cross-Channel premise that English history takes place only in England. There may have been a silent conspiracy to maintain the Hegelian unreality of the Hundred Years War.

However this may be, English history is that of a “polity”—as the term is defined here—affected by its situation in a penumbra of military power less politically organized. With the final withdrawal of the English from Aquitaine, there emerges a different period, datable from 1453 to 1688, wherein they do not possess the military power to intervene in the changing state system of Western Europe but both the military and the political role of their polity in Britain and the archipelago are profoundly altered. The Henrician Reformation, from this point of view, appears (in no unfamiliar light) as the revolutionary absorption of ecclesiastical jurisdictions by the jurisdiction of the English king and as part of a drastic remodeling of the royal authority—associated with the name of Thomas Cromwell—that, if it does not justify us in speaking of “Tudor absolutism,” enables us, as it did Englishmen in the next century, to think of the royal “state” as a fabric of power distinguishable from the polity of law and Parliament and capable of coming

into conflict with it. During Cromwell's years of power the Statute of Wales was enacted, assimilating that principality to the English nexus of county jurisdictions; and this has been associated with a fresh creation of Irish counties linked with a Dublin Parliament summoned by the wearer of the English crown. Brendan Bradshaw has studied this process as part of what he has termed "the Irish constitutional revolution";¹⁷ out of the dilemma between direct and indirect control of Irish lordships emerged a governmental structure capable of surviving and even modifying the conquests, rebellions, and colonizations of the sixteenth and seventeenth centuries. It made possible the formation of a settler nationalism in the eighteenth, and was taken over in the nineteenth and twentieth centuries by a modernized Irish nationality that has displaced all of the subnations of the older Ireland except the largely Scottish Unionists of the northern counties. Thomas Cromwell, therefore, extended the power of the state in two ways: absorbing Wales into the English polity and furthering the growth of a provincial polity in Ireland.¹⁸ It follows that a Welsh political nationalism must be different in character from an Irish.

Neither Ireland nor, though less unsuccessfully, Wales was adequately integrated with the Henrician church structure, while in the unabsorbed kingdom of Scotland there occurred a Reformation more of the Continental sort, in which a Calvinist ministry and nobility challenged the impaired power of a monarchy. When these facts are set beside the growth in the English church of what is termed Puritanism (a force capable of colonizing adventures across the Atlantic), a religious context is established for the troubles of the seventeenth century: the second great theme, after the growth of medieval government, of the English historiography. The thesis of the older Whig historians, both English and Scottish, is that the constitutional crisis was essentially a consequence of changes in the organization of military power: after the decline of feudal (including "bastard-feudal") warfare, the publicly financed professional army that emerged had to be controlled either through parliamentary taxation or by an absolute monarchy. It was the long-term good fortune of England that the issue was faced before a professional army was formed, and that the Civil War was fought by inspired amateurs for the control of a county militia. Whatever their shortcomings in a modern view, the Whig historians set the seventeenth-century crisis in a European context of changing military technology. The interpretations put forward by post-Whig and Fabian historians, who seek its causes deeper and deeper in changing relationships within English society, have paradoxically been far more Anglocentric. There is no inherent reason why this must be so; but the current reaction against social-change explanations does remind us that the English Parliament would not have met in 1641 if there had not been an armed rebellion in Scotland and that Parliament would not have demanded control of the militia in 1642 if there had not been another in Ireland. Explaining the remoter as well as the immediate causes of the crisis in terms of the social and

¹⁷ Bradshaw, *The Irish Constitutional Revolution of the Sixteenth Century* (Cambridge, 1979).

¹⁸ G. R. Elton, *Reform and Reformation: England, 1509–1558*, vol. 2 of the *New History of England* (Cambridge, Mass., 1977), 201–11.

political control of armed power may entail reverting to the superstructure; but the "British" dimension and the categories in which we try to understand it become more visible and demanding in such a framework.

An Ulster historian of Ireland has renamed "the Civil War"—a purely English nomenclature—"the War of the Three Kingdoms."¹⁹ If the apparatus of Stuart authority in all three is a superstructure, we must look for deeper causes within the three societies and their politics. The Irish rebellion of 1641 was no archaic affair of Gaelic warriors charging out of the mists, but the complex response of Old English and Old Irish populations to the threats that renewed English and Scottish settlement and intensified royal control presented to their position in the polity emerging from Bradshaw's "constitutional revolution."²⁰ In Scotland, it is arguable that the Covenanted revolution of 1637—crucial in the shaping of Scottish historiography—had less effect in transforming the conditions of national existence than the unsuccessful attempts of 1643–50 to determine the outcome of the English civil struggle.²¹ Here the Scottish polity in arms endeavored to secure a "British" settlement in church and state, and English historians to this day express horror at its presumption. If Scottish arms could have no role in England, a "British" solution might have to be imposed by the none too willing English; and the short-lived Cromwellian Union of the revolutionary 1650s was effected by the ascendancy in all three kingdoms of that most English of armies, the so-called New Model.

This enigmatic phenomenon²²—revolutionary movement? nascent professional force? instrument of colonization in Ireland?—must be allotted a crucial role in British history. The model presents "British history" in terms of (1) the central position of an English polity within a military penumbra of marches, (2) the independent existence of a Scottish polity and the evolution of marches into polities in Ireland. In this scheme, the military structure of the English polity takes on a special if elusive significance. There has until recently been no challenge to the Whig historians' thesis that no standing army was established in England in time to render ineffective the parliamentary thrust for control over its growth. Buckingham's troops who precipitated the debate over martial law in 1628 were many of them Irish and Scottish mercenaries;²³ the Civil War of 1642–46 was largely an affair of amateurs who learned the art of war as they went along; the radical regiments of 1647–48 insisted on their nonmercenary character. On the other side of the medal, however, Stephen Saunders Webb has recently proposed a sharp change of image. There was always a professional officer corps, he has insisted, drawn from the garrisons of the royal fortresses and reinforced by the regiments formed in the Dutch service. They appear as commanders dispersing the last effort of Northumbrian highland feudalism in 1569–70 and play a significant role in the

¹⁹ J. C. Beckett [Queen's University, Belfast], *The Making of Modern Ireland, 1603–1923* (New York, 1966), chap. 4.

²⁰ H. F. Kearney, *Strafford in Ireland, 1633–41* (Manchester, 1959); and Aidan Clarke, *The Old English in Ireland, 1625–42* (London, 1966).

²¹ David Stevenson, *The Scottish Revolution, 1637–44: The Triumph of the Covenanters* (New York, 1973) and *Revolution and Counter-Revolution in Scotland, 1644–51* (London, 1977).

²² Mark Kishlansky, *The Rise of the New Model Army* (Cambridge, 1979).

²³ Robert C. Johnson *et al.*, eds., *Commons Debates for 1628*, 2 (New Haven, 1977): 97, 105, 361, 365.

early phases at least of the Civil War. Webb has suggested that the New Model, and in general the armies of the Cromwellian Union, marked a significant stage in the growth of professionalization, since enough of their officers—notably those who followed George Monk from Scotland—survived the Restoration and were brigaded with the Cavalier commanders of the royal regiments to form a professional officer corps, crucial to the story he aims to tell. They garrisoned England for the restored but insecure Stuarts, and helped keep down the Covenanting guerrillas in Scotland and the tories and rapparees in Ireland. They were the hard core of James II's plans for a monarchy in the Continental style, and only a confrontation with the equally professional troops brought by William of Orange permitted the leaders of the polity to settle the campaign of 1688 by bloodless desertion instead of civil war.²⁴ But Webb, whose narrative has as yet reached no further than 1681, has already followed the drum across the Atlantic to Jamaica and Virginia; he plans to contend that the expansion of empire in America was the work of military men whose garrisons and fortresses could control settler as well as indigenous populations and so cannot be understood simply in terms of the agricultural and commercial growth of civil polities in the colonies. Empire, in the language of the model, was an affair of marches first and polities after.

Webb's thesis is clearly suggestive in the construction of a British history. He likes hinting that the role of the military in the governance of England itself may have been under-recognized; this will go down well with historians who wish to represent the Stuart and Whig polity as a dictatorship of its governing class, but less well with those who recognize the strength of the established contention that the governing class and the society it ruled were both profoundly amateur and civilian, their politics pervaded by mistrust of standing armies and even of militias. Webb is a historian of empire rather than of England, and the test of his future volumes will be their success in establishing a relationship between military frontier and civil society in American history down to Revolution and independence. From the viewpoint of the present essay, however, the value of his first book is that his revision of the military-civilian relationship enabled him to insist that England and the English colonies have a single and not a separable history: what is called "the colonial period of American history," he affirmed, was in reality "the period of Anglo-American empire."²⁵ Some of his insights into empire and polity are applicable to that transformation of relationships within the archipelago that led to the creation of modern Britain.

If James II appears in Webb's scenario a serious and formidable figure, the architect of the Anglo-imperial officer corps as it took shape after the Restoration, his supplanter, William III, plays a revolutionary role in the transformation of English and British military power. He associated English fleets and armies with those of the House of Orange and its allies, restored them to European warfare, and committed England to what proved a cycle of French wars lasting from 1688 to

²⁴ Webb, *The Governors-General: The English Army and the Definition of the Empire, 1569–1681* (Chapel Hill, 1979).

²⁵ *Ibid.*, 466.

1815. To this end the parliamentary hatred of standing armies had to be reconciled with the maintenance of a permanent military force, and the resources of public finance had to be made capable of supporting it. The great innovations of the "financial revolution" not only solved this problem but did so in ways that stabilized English politics, in the sense that a highly effective oligarchy of landed and financial magnates was able to rule in the teeth of the bitter hostility, but implied acceptance, of rural and urban oppositions. The Whig polity thus born transformed social relations and made England capable of an imperial—which included a British—role in the archipelago, Europe, America, and India.²⁶ Here again the historiographical paradox fundamental to this essay becomes patent: Whig rule stabilized parliamentary politics and made possible both Britain and its empire, but both Whigs and Namierites have recognized only the history of Parliament as *verità effettuale* and have made it the third great theme of English historiography.

William III was obliged to campaign on the British frontiers against James's provincial commanders: Claverhouse in Scotland, Tyrconnel in Ireland (Andros in New England was dealt with by the locals).²⁷ In all three cases, the relation of march to polity should be observed. In Scotland, the Revolution began a process whereby the lowland kingdom, itself increasingly parliamentary and oligarchic in its politics, sought closer association with the stability and expansion of the Whig polity and ultimately determined after much hesitation to seek an incorporating union that the English accepted only when convinced that the alternative must include military control.²⁸ The "Britain" of 1707 created no new nationality; it was the fruit of an English desire for stability and a Scottish pursuit of economic modernization. The Scottish marches—the highland zone where military and tributary techniques of control had long alternated—had to be subjected to the needs of this process, and in the succession of Webb's "governors-general" appear Field-Marshal Wade and the Duke of Cumberland, engaged in what any Roman would instantly have recognized as the reduction of a new province; the conquest of the Highlands, however, was a North British rather than a merely English enterprise. In Ireland, William's campaign was the last of the series of military expropriations and settlements, which since Elizabeth I had been converting marches into colonies and colonies into polities operating within the frame of a subordinate parliamentary system. The siege of Londonderry symbolizes the birth of the Scots-Irish settler nation, the treaty of Limerick that of the Protestant Anglo-Irish (as well as the diaspora poetically called the "wild geese"), and the first manifesto calling for Anglo-Irish parliamentary autonomy appears in 1698.²⁹ It is fairly untypical, however, and the politics of eighteenth-century Ireland are those of a locally administered province

²⁶ For a further treatment of this theme, see my "1776: The Revolution against Parliament," in J. G. A. Pocock, ed., *Three British Revolutions: 1641, 1688, 1776* (Princeton, 1980).

²⁷ J. G. Simms, *The Williamite Confiscation in Ireland, 1690–1703* (London, 1956); and David S. Lovejoy, *The Glorious Revolution in America* (New York, 1972).

²⁸ P. W. J. Riley's *The Union of England and Scotland: A Study of Anglo-Scottish Politics in the Early Eighteenth Century* (Manchester, 1978) may be seen from its subtitle to take a micropolitical rather than a macrohistorical view of the "episode."

²⁹ William Molyneux's *The Case of Ireland Being Bound by Acts of Parliament in England Stated* (Dublin and London, 1698).

of the Whig empire of parliamentary patronage—a phrase not inapplicable, with modifications, to those of Scotland between 1707 and 1832. There is a significant military role in the politics of each, and both Scots and Anglo-Irish are being recruited to the officer corps of the Whig empire; but the substitution of polity for march is virtually completed. Irish independence in the twentieth century was to be achieved not by rebellion but by revolution.

“British history” in the eighteenth century has in the fullest sense an Atlantic dimension. Each major step in the consolidation of the archipelago under a single parliamentary monarchy—1689, 1707, 1745, and 1801—was undertaken in the context of one or other of the wars with France for ascendancy in Europe, to which William III had obliged his subjects to commit themselves; and each of these until 1783 enlarged the scope of British and French warfare in America. Webb obliges us to consider the growth of Anglo-American empire as an affair of marches as well as polities; but, whatever the role of military men in organizing the extension of imperial power, or of grievances against their doings in bringing the colonists to revolution, there seems no alternative to viewing American independence as the outcome of a process whereby colonial polities became capable of acting as states and taking over the management of their own growth as an empire. Population growth—in which the Scots-Irish settlement of Appalachia³⁰ is a notable feature—is now taking its place alongside social and ideological formation as a cause of the Revolution; we are returning to the perspective of Benjamin Franklin’s prediction that the center of empire was being transferred across the Atlantic, which moved some English and Scottish observers to the conclusion that, if Britain could not control the growth of American empire, the only course was to separate entirely from it.³¹ The ideology of free trade, like the construction of an Anglocentric historiography, was a means of saving England from the fate of Rome. On the American side of the equation, the ultimate question in view, as we peer through the lens of Webb’s telescope, is how far empire beyond the Alleghenies was created by the march-building activities of a military elite now wholly American, and how far by polities of settlement expanding so fast that they overtook their marches at a single step and incorporated them in their politics before their work was half done. There are not many proconsuls in U.S. history, east of the Philippines.

The creation of new polities, and even new nations, along the fluctuating frontiers of empire can be seen continuing. It is time American historians ceased imagining the Loyalists as perishing tearfully by the waters of Babylon; the history

³⁰ For a recent reassessment of the Scots-Irish role in American history, see Robert Kelley, “Ideology and Political Culture from Jefferson to Nixon,” *AHR*, 82 (1977): 531–62. This may be the place to mention the recent neo-Celtic interpretation of the history of the mountain South: Forrest McDonald and Ellen Shapiro McDonald, “The Ethnic Origins of the American People, 1790,” *William and Mary Quarterly*, 3d ser., 37 (1980): 179–99, and Forrest McDonald and Grady McWhiney, “The South from Self-Sufficiency to Peonage: An Interpretation,” *AHR*, 85 (1980): 1095–1118. I think the authors exaggerate (and cite me as endorsing) the extent to which what was not English was Celtic.

³¹ The most energetic proponent of this view was Josiah Tucker; see George Shelton, *Dean Tucker and Eighteenth-Century Economic and Political Thought* (New York, 1981). Also see my “Hume and the American Revolution: Dying Thoughts of a North Briton,” in David Fate Norton *et al.*, eds., *McGill Hume Studies* (San Diego, 1979), 325–44.

of the Revolution is incomplete without the history of Canada,³² which consists partly of societies founded by them and wholly of polities that resolved their relations with the British monarchy along lines other than those followed by the United States. British history in the eighteenth century must be thought of as the history of four realms, rather than the traditional “three kingdoms” of the archipelago; the North American realm is disrupted and reconstituted by the secession of the thirteen colonies, but British history on the American continent does not thereby come to an end. Rather than relegating it to a separate category of “colonial” or “imperial” history, which would imply a surrender to Anglocentricity, we must continue to suppose an archipelagic and Atlantic world—known to contemporaries simply as “the empire”—which was disrupted but not destroyed when a segment of it took off and entered a continental dimension of world history into which British history was not structurally attracted. The history of empire as exercised by the British state and people from this point becomes increasingly the history of empire in India and in other regions to which it was attracted by global commerce and naval power;³³ only where it established colonies of settlement is this empire’s history identical—although it remains everywhere profoundly relevant—to that of the “British” peoples and their political culture. The so-called first British empire is deeply discontinuous with the second.

The conclusion drawn by some contemporary analysts of the American trauma was that the existence of subordinate legislatures in a parliamentary empire was a contradiction in terms; legislative sovereignty was indivisible. The reality was to prove more complex. Before the War of Independence ended, a “patriot” movement among the Anglo-Irish set about remodeling the relationship between Westminster and Dublin: the start of twenty years (1780–1801) of confused and sometimes violently rebellious politics occasioned by the changing relations between the three Irish political nations—Anglo-Irish, Scots-Irish, and restructured Catholic Irish.³⁴ With the legislative union that created the United Kingdom of Great Britain and Ireland, the early modern period of British history may be said to come to an end. It appears the culmination—in the event impermanent—of a process of consolidating the archipelago as a single kingdom, polity, or empire-state; looked at more narrowly, however, it may be seen as one more response to the problem of polity and empire, with subordinate legislature taking the place of march. No role had been found for American colonial assemblies, and they were now the legislatures of states; no role had been found for the Irish parliament, and its members were now returned to sit at Westminster. It did not follow that there were no cases in which a subordinate legislature might not exercise as much local control as Westminster desired to see exercised, and even within the United Kingdom, to say nothing of the archipelago, experiments of this kind could still be envisaged. Unlike the Kingdom of Great Britain, however, the United Kingdom was required

³² A bibliography of Canadian history would be out of place here, and I am unable to name any general history of the North American continent in the Revolutionary epoch or after. Canadian historiography, however, is rich in speculation on the problems of national and continental identity.

³³ Vincent Harlow, *The Founding of the Second British Empire, 1763–1793*, 1 (London, 1952).

³⁴ R. B. McDowell, *Ireland in the Age of Imperialism and Revolution, 1760–1815* (Oxford, 1979).

to face the question whether it could function enduringly as a polity; in the end, it could not, disintegrating into two or three. We are now at the point where we can see why it has no unified historiography.

THE MODERN PERIOD OF OUR "BRITISH HISTORY" is dominated by the creation and disintegration of the union of 1801–1922 (the latter date seems closest to *verità effettuale*, although the union was legally transformed in 1920). So at least we must say if the field is the formation of peoples and polities within the Atlantic archipelago and radiating from it; but, quite naturally, the problems of this process, and even of the union itself, were not always at the center of attention for the politicians at Westminster or even their electorates. As was once said of the empire, the union and even Britain itself were created by the English in a fit of absence of mind. This is the period of industrialization and agrarian crisis, of Indian and maritime empire, of the transformation of European and Asian-American power relations, and of the British relation to them; and it is arguable that the perception of "Britain" has never been more than part of Westminster's perception of its relation to the world. Since it is our business to emphasize and illuminate the former perception, we begin by noting that the remodeling of the relations between electorates and Parliament, whose central issue was the transformation of the historic English structure of shire and borough, was initiated as a consequence of the Union, by the removal of Catholic electoral disabilities in 1829. This mainly Irish measure had British consequences of vast import. It furthered the English and Scottish electoral reforms of a few years later; it brought about a local, but significant, crisis of identity among the clergy of the Church of England³⁵ when some of them realized—as fewer had cared to do in 1707—that the political structure or state in which they lived was no longer that of which they were the church by law established. But Emancipation led also to Daniel O'Connell's attempt to procure repeal of the Union through the organization of mass meetings; and, when this failed, there ensued both the formation of a conspiratorial revolutionary tradition (it had roots in the eighteenth century) and the growth of an Irish parliamentary party that by degrees became capable of demanding a separate parliamentary polity.³⁶ We may think of the English reforms as the successful adaptation of Whig aristocracy to new conditions; but in Ireland we must notice processes that transformed the structure of society and rendered it increasingly political. These arose from the politicization of the clergy and laity, which Emancipation made possible, and the irony of Union history was to be the growth of a counterpolity formed in the opposition to Union itself. The creation of new political nations had not come to an end with the age of the marches.

The Home Rule crisis of 1886, brought about by some apparently modest proposals for parliamentary devolution, was the fruit of improved techniques of

³⁵ R. W. Church, *The Oxford Movement: Twelve Years, 1833–45* (London, 1891), ed. Geoffrey Best (Chicago, 1970).

³⁶ Conor Cruise O'Brien, *Parnell and His Party* (Oxford, 1957); F. S. L. Lyons, *Charles Stewart Parnell* (Oxford, 1977); and Owen Dudley Edwards, "Ireland," in Edwards *et al.*, *Celtic Nationalism* (New York, 1968).

political mobilization in Ireland; it also witnessed the first signs of counter-mobilization in Belfast, where a popular Unionism was now almost a century old. In England, where the structure of popular politics was also changing, the crisis brought political issues as close as they have ever been to becoming "British" issues. The Conservative party was officially renamed the Unionist party, and for about thirty-five years (1886–1922) sought to commit the English to the view that their only future as a nation lay in the creation of a greater union and empire.³⁷ The view was not deeply held and was speedily eroded; outside Ireland, unionism and imperialism are remembered chiefly as marking an episode in the history of English party politics. But in the context of British history, the episode of their conjunction opens up vistas that need to be explored.

During the brief period of imperialist politics, empire was perceived in the first instance as union; in the second, as those great administered provinces, in India and elsewhere, that are misnamed "colonies" because their populations were not established by the processes of colonization and settlement. In their histories there is a "British" episode, often of great importance, but they do not form part of the "British history" being explored here, which for a time shared a political and historical context with them. Lastly—and it usually was lastly—empire was perceived as consisting of those colonies in the true sense, established in North America, the South Pacific, and southern Africa, by emigration from all parts of the Atlantic archipelago (although Canadian colonization was in large part an incident in the general process of American immigration and the side of the line on which one settled was often determined by local accident). The main streams of British and Irish emigration flowed out of the empire into the United States (an empire of another sort); and it is of some significance that British immigrants did not become hyphenated Americans, that the Irish diaspora retained its national identity and provided powerful support for Catholic Irish nationalism, but that the statistically significant Scots-Irish emigration has never been mobilized to support the Protestant homeland. The formation of national identities in the colonies of settlement has an ethnic dimension, but that is a complex story differing sharply from the American experience.

Toward these colonies of settlement, the government of the United Kingdom adopted an attitude with which the American trauma of the eighteenth century had plainly something to do. Once local polities had developed that military governors could no longer control, they were encouraged to adopt constitutions of responsible government. From Westminster these were seen as ensuring that, should a colony opt for republican independence, the separation would be as painless as possible while at the same time, should one become involved in a war of its own making, the British commitment to support it could be limited.³⁸ The creation of quasi-independent polities continued the imperial tradition of limited and indirect

³⁷ It might be suggested that two major works of historiography arose from this movement: Sir John Seeley's *The Expansion of England* (Cambridge, 1883) and Laurence H. Gipson's *The British Empire before the American Revolution*, 10 vols. (Caldwell, Idaho, 1963–70). For Seeley, see Deborah Wormell, *Sir John Seeley and the Uses of History* (Cambridge, 1980).

³⁸ A. P. Thornton, *The Imperial Idea and Its Enemies* (New York, 1959).

control. In the event no dominion became a republic while controlled by its British population, and no colony was in a position to involve Britain in undesired war with its neighbors. Oceanic distance ensured this result in the South Pacific;³⁹ only in South Africa did the British settlements find themselves involved in the problems of a military frontier, created by Afrikaner emigration to the continent's interior space. The South African War of 1899–1902, designed to end the frontier and incorporate the Afrikaners in a larger polity, was brought about by imperial policy rather than settler politics; and it was accurately foretold that the dominion that it created would be a failure unless the English-speaking component could predominate over the Afrikaans (an “African” history of South Africa would be another story still).⁴⁰ It was also the first of a series of global wars in which the settler dominions joined to ensure the strategic unity of a naval and supranational empire and in which the nationalism of a “Greater Britain” sought but did not find an enduring and effective political expression. The exhaustion of this enterprise brings “British history” to its latest and postimperial phase.

The symbiosis of empire and union persists to the end. We have seen that the so-called first British empire was essentially an extension across the Atlantic of the complex of marches and polities by which the archipelago underwent consolidation, that its achievement was an effect of the Whig political system's dual capacity for domestic stability and imperial power, and that, from 1689 to 1801, each step in the consolidation of union—as in the creation of an Anglo-American empire which opted for independence—coincided with one of the series of Anglo-French wars by which the Whig polity achieved power in the world. In this series, however, the Union of 1801 is something of a delimiting case. It was an aftereffect of the American disruption of empire, and in the wars against the French Revolution and Empire the old world of Anglo-French rivalry came to an end. The British victories in the Whig wars entailed the acquisition by naval and commercial means of a commanding detachment in relation to Europe; sea power and subsidies could influence the outcome of Continental wars without too great an involvement in them—the “modern Carthage” need not march on Rome (at least before 1814). During the nineteenth century this commanding position was eroded. There arose in Europe a complex of powerful centralized states, whose conflicts could not be controlled by the traditional Whig techniques; Japan and America created a similar situation in eastern Asia and the western Pacific. In the world wars of the twentieth century the substance of British naval and imperial power was dissipated; the expeditionary forces of the dominions could not save a crumbling oceanic structure; and in the outcome the United Kingdom was obliged to seek admission to a Europe it could no longer control or avoid.⁴¹ We may see an analogy with the Scottish decision to seek Union in 1707; but “Europe,” unlike “Great Britain,” was not an expanding empire, but in its turn an attempt to escape domination by superior extraneous strength.

³⁹ Geoffrey Blainey, *The Tyranny of Distance* (Sydney, 1966).

⁴⁰ See Monica Wilson and Leonard Thompson, eds., *The Oxford History of South Africa* (Oxford, 1969).

⁴¹ See Nicholas Tarling, “The Wars of British Succession,” *New Zealand Journal of History*, 15 (1981): 24–34.

If the steps by which the United Kingdom was created can be synchronized with the wars by which empire was attained, those by which it disintegrated may be synchronized with the wars by which empire was lost. The Ulster crisis of 1912–14—now seen as part of a larger crisis of parliamentary authority, “the strange death of Liberal England”⁴²—was superseded by the German invasion of Belgium. The Easter Rising of 1916 occurred shortly before the first great attempt to break out of trench warfare. The Irish Revolution of 1919–21 was furthered by the proposed imposition of conscription on an Ireland still part of the United Kingdom, and was fought out in a context of postwar demobilization. The Irish Free State was finally transformed into a republic following Irish neutrality in the Second World War. The insurgency that began in 1969, and continues to challenge the existence of the United Kingdom as modified in 1920–22, has coincided with the stages of British entry into the European Economic Community, as have the movements for Welsh and Scottish devolution that have yet to produce any crisis in the political unity of the larger island. The connections between loss of empire, assimilation to Europe, and loss of Union are without doubt confused and contradictory, but they seem to exist; and, if a political “British history” is defined as the creation of an offshore empire, a case might be made for holding that the history of Britain is coming to an end and is about to be written by the owls of Minerva. But the themes used in constructing this essay seem to yield further hypotheses.

Ireland is the counterpolity of modern British history, as the United States is that of the early modern period. A political order created by assimilation to a parliamentary kingdom was used to establish a polity outside it. As the American Revolution is the exception among the revolutions of the late eighteenth century, the Irish is the exception among those of the early and middle twentieth. In no other country of Western Europe was a new state created by revolution in the aftermath of the First World War; yet, because this revolution was neither communist nor fascist, it does not interest intellectuals and has received little attention from historians. They may have missed an instructive case. On the one hand, modern insurgency warfare is in significant degree an Irish gift to the world; its theory was elaborated (at the passive-resistance stage) by Arthur Griffith,⁴³ its symbolic action by Patrick Pearse,⁴⁴ its practical conduct by Michael Collins,⁴⁵ and the hunger-strikers of the Maze were continuing the politics of moral enormity as these words were being written. On the other hand, the conduct of an urban guerrilla war in 1919–21 and between its practitioners in 1922 did not leave the successful activists in uncertain command of a single-party state; there emerged instead a stable parliamentary democracy, which we must explain by saying that a structure defining the relation of constituencies to elected assembly, which had been established by parliamentary union, persisted to legitimize the Free State

⁴² George Dangerfield, *The Strange Death of Liberal England* (New York, 1935).

⁴³ Arthur Griffith, *The Resurrection of Hungary* (Dublin, 1904).

⁴⁴ F. X. Martin, ed., *Leaders and Men of the Easter Rising: Dublin, 1916* (London, 1967); and William Irwin Thompson, *The Imagination of an Insurrection: Dublin, Easter 1916* (New York, 1967).

⁴⁵ Rex Taylor, *Michael Collins* (London, 1958); and Charles Townshend, *The British Campaign in Ireland, 1919–21* (Oxford, 1980).

and the Republic. The assimilation of peoples to one another, the provision of forms within which conflicts are fought out, remains a theme of archipelagic history.

The elected representatives of twenty-six counties had seceded from the Parliament of the United Kingdom to form Dail Eireann and claim the authority of a state; the insurgency of 1919–21 was fought to reinforce their authority, the civil war of 1922 to clarify it. When the United Kingdom conceded their claim without conceding full independence, it both consented to its own truncation and reverted to the policies of the imperial frontier. The Republic has never quite lost the character of a secondary and concessionary polity. When the six counties of Northern Ireland remained within the Union but acquired a subordinate parliament at Stormont, there were Unionists who feared the consequences. Stormont neutralized the Northern capacity to organize revolutionary mobilization to resist exclusion from the United Kingdom, but continued the clear implication that the Protestants' will to remain within the Union was stronger than the Kingdom's will to retain them. The second United Kingdom "of Great Britain and Northern Ireland" was therefore anomalous in a way the first had not been, and the aim of the present insurgency is to dissolve it by dividing the various wills that currently hold it together.⁴⁶ It is the uncertain meaning, and the uncertain coincidence, of the terms "Britain" and "United Kingdom" that render this strategy possible; but the terms "Ireland" and "Republic" are not necessarily more clear or coincident. The "British" or archipelagic history outlined in this essay has an ideological consequence: it reveals the ideological falseness of the claim of any state, nation, or other politically created entity to natural or historical unity.

Organized in political categories, it has been a "death of the past";⁴⁷ it has used the adjective "British" to define a process of the making and unmaking of polities and political nations and does not presuppose that history records the past or continuous existence of any structure or combination of structures; it is concerned with *conjonctures* and *ruptures* rather than *durées*. It has not been a history of Union any more than of dis-Union; but the Anglo-Irish relationship, where these forces composing the process have most conspicuously interacted, has provided the stress zone or "seismic rift"⁴⁸ around which it has been constructed. This has been a selective emphasis; there are themes in Welsh, Scottish, and even English history—the relation between the agricultural and commercial "political nation" and the moorland north whose water power and minerals originated the Industrial Revolution is not, after all, foreordained—that have not been touched in this essay. But a framework has been provided—that of interacting and expanding structures of jurisdiction and state power, creating polities and nationalities as they grow and change—within which a multinational and at the same time antinational history might acceptably be written. From a historiography that recognizes that political

⁴⁶ This is a field in which to be detached is to take sides; suffice it to mention Conor Cruise O'Brien, *States of Ireland* (New York, 1972), and Richard Rose, *Governing without Consensus* (London, 1971). Historiography has not yet replaced the interpretive essay or report.

⁴⁷ J. H. Plumb, *The Death of the Past* (Boston, 1970).

⁴⁸ A phrase borrowed from Lawrence Stone. "The Results of the English Revolutions of the Seventeenth Century," in Pocock, *Three British Revolutions*, 23–80.

mobilization is a most powerful engine in determining political and historical subjectivity, it would be perfectly possible to turn to one that deals in the conflicts within productive and distributive structures or to one that reflects on the enduring objectivities of geographical and material conditions. There are many histories that ought to be written of the Atlantic archipelago and its Atlantic and Pacific extensions.

Thomas Jefferson

The Virtue of Aesthetics and the Aesthetics of Virtue

LEE QUINBY

FOLLOWING A PARISIAN SUMMER of shared friendship in 1786, a “solitary and sad” Thomas Jefferson wrote to Maria Cosway of his pain at departing and his hope for a reunion. The result was the now famous dialogue in which Jefferson depicted his head and heart as at odds over whether the pleasure of friendship warrants the pain of parting. In this dialogue, the Head condemns friendship as “but another name for an alliance with the follies and the misfortunes of others.” In refutation, the Heart, wary of the Head’s calculating self-interest, propounds what it sees as their respective roles: “When nature assigned us the same habitation, she gave us over it a divided empire. To you she allotted the field of science, to me that of morals.” Friendship, the Heart insists, belongs to the Heart’s domain.¹

Scholars have long agreed that this letter discloses not only Jefferson’s attachment to Cosway but also his approach to morality. At the same time, however, they have disagreed considerably over which side actually prevails in the disputation. The traditional interpretation presents a Jefferson in whom reason holds sway over an obedient sentiment. According to Merrill Peterson, “the head (Jefferson) coolly puts the heart (Maria) in its place” by the dialogue’s end. Reason and sentiment may divide life; yet for Jefferson “one was the master, the other the servant.” Julian Boyd has concurred, arguing that Jefferson was, despite this struggle with emotion, “the man to whom reason was not only enthroned as the chief disciplinarian of his life but also, as revealed in the nature of his response to its commands, was itself a sovereign to whom the Heart yielded a ready and full allegiance, proud of its monarch and happy in his rule.”²

Conversely, Garry Wills has recently contended that the Heart resoundingly triumphs over the Head. Wills’s bold and innovative reassessment in *Inventing America* corrects the traditional assumption that Jefferson held unswervingly to the

I am pleased to acknowledge an intellectual debt to Lester Cohen and Patrick Bidelman and wish to thank them for their encouragement and, on a more concrete but equally essential level, their editing advice. The present version of this essay has also benefited from the comments of the anonymous referees for the *American Historical Review*.

¹ Jefferson to Cosway, October 12, 1786, in [Jefferson] *The Papers of Thomas Jefferson*, ed. Julian P. Boyd [hereafter, *Jefferson Papers*], 19 vols. (Princeton, 1950–), 10: 443–53.

² Peterson, *Thomas Jefferson and the New Nation* (New York, 1970), 349; and Boyd, Editor’s Comment, *Jefferson Papers*, 10: 453n.

superiority of reason over sentiment. The Heart that triumphs over the Head is, however, a mathematical, empirical instrument in Wills's interpretation: his sentimentalist Jefferson remains above all a quantifier, one who spoke the language of measurable happiness, of scientifically verifiable sublimity, and of rigidly calculable emotion.³ Wills's Jefferson employed a "science of morality."⁴ Although Wills's challenge to older views on Jefferson is salutary, his conclusion in effect contradicts the spirit of the Heart's own admonition to square the circle through science but to right wrongs through morality.⁵ In other passages Jefferson confirmed that he indeed held to a "divided empire" concept of science and morality and that, furthermore, he viewed as menacing the notion that morals were a matter for scientific calculation. He wrote, for example, to his nephew Peter Carr in 1787 that "He who made us would have been a pitiful bungler if he had made the rules of our moral conduct a matter of science."⁶

Given this "divided empire" approach to science and morality, what, then, did provide direction toward virtue in Jefferson's schema?⁷ Rather than a morality ruled by either reason or science, Jefferson adumbrated what I call an aesthetics of virtue, a fusion of art and morals, whereby reflective beings are capable of discerning the path to virtue through aesthetic experience. For Jefferson, in short, aesthetics charted avenues of direction for virtuous conduct. In his letter to Cosway, he concluded the dialogue not on the triumph of either the Head or the Heart but, instead, on a note of virtuous harmony between the two. Closing the debate on what he designated as "a favorable proposition," Jefferson thus approved a compromise in which the Heart calls upon the Head to help furnish amusement for

³ Wills has argued for a "Hutcheson scheme of things" in Jefferson's works, in part based on Jefferson's and Francis Hutcheson's similar concern for mathematical calculation. Although Jefferson employed an algebra of sorts to calculate "what constituted a mulatto" and carefully recorded everything from crop rotations to population expectations, it is stretching the point of resemblance to say that such formulas are the same as Hutcheson's Moral Calculus. Wills has remarked that Jefferson's page with the formula on mulattoes "looks like a page of Hutcheson's formulae for the moment of virtue"; Wills, *Inventing America: Jefferson's Declaration of Independence* (New York, 1978), 298. Of course it does, but their resemblance is that of appearance, not subject matter. None of Jefferson's meticulous record-keeping indicates any attempt to formulate algebraically moments of virtue, happiness, morality, and the like. For a recent refutation of Wills's links between Jefferson and Hutcheson (on grounds other than mine), see Ronald Hamowy, "Jefferson and the Scottish Enlightenment: A Critique of Garry Wills' *Inventing America*," *William and Mary Quarterly*, 36 (1979): 503–23.

⁴ Wills, *Inventing America*, 164.

⁵ The Heart's "divided empire" exhortation reads as follows: "To you [nature] allotted the field of science, to me that of morals. When the circle is to be squared, or the orbit of a comet to be traced; when the arch of greatest strength, or the solid of least resistance is to be investigated, take you the problem: it is yours: nature has given me no cognisance of it. . . . Morals were too essential to the happiness of man to be risked on the uncertain combinations of the head. She laid their foundation therefore in sentiment, not in science." *Jefferson Papers*, 10: 450.

⁶ Jefferson to Carr, August 10, 1787, in *Jefferson Papers*, 12: 14. Three decades later Jefferson expressed a similar view—in somewhat bleaker terms—to a different correspondent, admitting that, "from the experience of the last twenty-five years, . . . morals do not of necessity advance hand in hand with the sciences"; Jefferson to M. Correa, June 28, 1815, in [Jefferson] *The Writings of Thomas Jefferson*, ed. H. A. Washington, 8 vols. (Washington, 1851–54), 6: 480.

⁷ A brief distinction in terms: Although Jefferson is not systematic in his usage, in general he viewed morality as fundamental to the human constitution, with each individual possessing an innate Moral Sense capable of ascertaining right and wrong. Virtue, the consequence of an act that has effected happiness for others, according to Jefferson, is a form of moral excellence. Although virtue depends upon morality, it may be seen as a higher point along a continuum. The distinction between morality and virtue is, of course, one of degree. For the significance of this distinction for Jefferson, see page 352, below.

the Cosways whenever the friends meet again and promises to “seize any occasion which may offer to do the like good turn for [the Head] with Condorcet, Rittenhouse, Madison, La Cretelle, or any other of those worthy sons of science.”⁸ Such a gesture does not merely exemplify an aesthetics of virtue, it is paradigmatic of it. To demonstrate this contention that aesthetics provides the key to virtue for Jefferson, I shall describe the theoretical model I am employing and locate the model’s major literary antecedent; I shall then discuss Jefferson’s particular expression of it.

BRIEFLY OUTLINED, what I call “aesthetics of virtue” is a dynamic model comprised of two fundamental dialectics: the first is between the Heart’s domain (sentiment) and the Head’s domain (the combined mental faculties of memory, reason, and imagination), and the second is between humanity and nature. In ideal form, these two dialectics act in conjunction with each other, enabling human beings to attain the stature of *homo aestheticus*, a species capable of creating and realizing its full potential through the interplay of body and mind, humanity and nature.⁹ The evolutionary dimension of *homo aestheticus* is consistent with Jefferson’s view of history as dependent upon human creation and as potentially—but not necessarily—progressive.¹⁰ I have derived the phrase “aesthetics of virtue” from the persistence in Jefferson’s language of aesthetic images and metaphors, from his belief that humans possess “an innate sense of what we call *the beautiful*, ” and, finally, from his assertion that “the nobler kinds” of art are those “which arouse the best feelings of man, which call him into action, which substantiate his freedom, and conduct him to happiness.”¹¹

In the first dialectic, the domains of Head and Heart confront each other, and each undergoes transformation in the process of that confrontation. Jefferson presented precisely such a transformation in the final passage of the Head-Heart dialogue, wherein memory and imagination facilitate a fusion of sentiment and reason in regard to his friendship both with Cosway and with his comrades in science. Although attained through a process that is at times painful, this dialectical synthesis eventually yields personal tranquillity, a felt pleasure in which human beings experience simultaneous delight in harmony and beauty. In the second dialectic, nature acts as both a teacher and a tool of virtue. Nature the teacher of

⁸ Jefferson to Cosway, October 12, 1786, in *Jefferson Papers*, 10: 452.

⁹ For the term *homo aestheticus*, I am indebted to Stefen Morawski, “Introduction,” in Lee Baxandall and Stefen Morawski, eds., *Marx and Engels on Literature and Art* (St. Louis, 1973), 10.

¹⁰ Lester H. Cohen has argued that the historians of the American Revolution employed a new mode of analysis, which he has designated as a “transformation of Natural Law into a historical process,” an analysis that released them from static, providential historiography and more easily enabled them to justify the Revolution by fusing necessity and expediency. He has described Jefferson’s writing, by contrast, as “more an uneasy mixture of the historical and the transcendent than a synthesis of them.” I see Jefferson, however, as deeply immersed in the kind of processive theory Cohen has so insightfully delineated. See Cohen, *The Revolutionary Histories* (Ithaca, N.Y., 1980), 134–46, 256n.

¹¹ Jefferson to Thomas Law, June 13, 1814, in *The Writings of Thomas Jefferson*, 6: 348–51; and Jefferson, *Notes on the State of Virginia* (1787), ed. William Peden (Chapel Hill, 1954), 65.

virtue provides a canvas that appears wild and irregular if seen in fragments but that emerges as placid and harmonious when viewed as a whole; and nature the tool of virtue provides resources for human activities ranging from agriculture to the fine arts. To Jefferson, the American continent in particular, with its vast supply of land, literally beckoned its populace to interact with nature and reap a product synthesizing the domestication of the land and its people. Because he saw the potential for such a synthesis, he believed that “those who labour in the earth are the chosen people of God, if ever he had a chosen people, whose breasts he has made his peculiar deposit for substantial and genuine virtue.” Hence, Jefferson’s advocacy of agrarianism represents less his wish to return to some kind of Golden Age than an acute assessment of his country at that time and its probable future when land would no longer be available and manufacture would become a necessity. “While we have land to labour then,” he urged, “let us never wish to see our citizens occupied at a work-bench, or twirling a distaff.”¹² Believing that nature cultivates virtue, he advocated agrarianism as the best means to strengthen each new citizen’s resistance to the inevitable encroachment of corruption.

This complex dynamic casts light on the principles by which Jefferson sought to fashion virtue throughout his writings. Perhaps the most striking feature of these principles lies in the subtle diminution of reason as requisite for virtue and the correlative elevation of imagination and sentiment. To be sure, Jefferson neither ignored nor denigrated reason. But, as his letter to Cosway makes clear, he refused to give it more weight than moral sentiment, for only in the dialectic between sentiment and all of the mental faculties could virtue be cultivated. In addition, Jefferson’s writings stress the means of gaining virtue—that is, the process and struggle itself—nearly as much as the desired result. Thus, his place in the history of ideas appears to be more ambiguous than is usually appreciated. Rather than solely an exemplum of the neoclassical rage for order, harmony, and regularity, Jefferson was almost as enthusiastic as were the Romantics for disorder, disharmony, and irregularity. Embracing these apparently contradictory ideals, Jefferson attempted to advance the cause of virtue by enlisting romanticism in the service of enlightenment.

Although Jefferson assimilated ideas eclectically from sources that literally spanned centuries, the most significant antecedent of his aesthetics of virtue is the work of Anthony Ashley Cooper, Third Earl of Shaftesbury. Shaftesbury, commonly credited with having sown the seeds of romanticism, was immensely popular in Jefferson’s time and, with the philosophers of the Scottish Enlightenment, contributed enormously to the dissemination of the eighteenth-century concept of an innate Moral Sense, a hallmark of Jeffersonian thought.¹³ Shaftesbury’s fusion

¹² Jefferson, *Notes on the State of Virginia*, 164–65.

¹³ Wills has astutely recognized the intimate link between Jefferson’s Moral Philosophy and his public writings. Although others have noted the impact of the Scottish Moral Sense School—Peterson has pointed to Hutcheson as well as to Lord Kames, Gilbert Chinard gave Lord Kames “first rank,” and Bernard Sheehan has cited Kames as the major influence on Jefferson’s environmentalism—on Jefferson’s thought, no one has so thoroughly connected Jefferson’s Moral Philosophy to the Declaration of Independence as has Wills. See Wills,

of ethics and aesthetics prompted admiration and emulation for a century following his death, as is evident in the works of Burke, Schiller, Goethe, and Kant.¹⁴

Jefferson's notebooks reveal a profound respect for Shaftesbury's work. When in 1776 he prepared his "Resolutions for Disestablishing the Church of England and for Repealing Laws Interfering with Freedom of Worship," which eventually became the Bill for Religious Freedom of 1786, Jefferson recorded virtually word for word a lengthy passage from Shaftesbury's *Characteristics of Men, Manners, Morals, Opinions, Times* (1711). Not only does the overall gist of the final Bill for Religious Freedom fundamentally affirm Shaftesbury's dicta regarding the necessity of separating Church and State, but several of its passages also resemble Shaftesbury's phraseology.¹⁵ Even more significant, however, are resemblances in the general categories of thought found throughout their respective writings. Just as Jefferson stood in basic agreement with Shaftesbury on the separation of Church and State, so too did his aesthetics of virtue reflect the ambience of ethics and aesthetics wrought by Shaftesbury.¹⁶ An examination of Shaftesbury's treatise on virtue helps show the contours of Jefferson's thought on the subject.

Throughout the collected essays that make up *Characteristics*, Shaftesbury encouraged the cultivation of virtue by employing the language of aesthetics to underwrite his moral convictions. This link is more than merely metaphorical. Aesthetics embraces morality for Shaftesbury because both art and virtue, in his view, strive toward the harmony inherent in humanity's greatest teacher—nature, where "symmetry and proportion" are found and wherein "Virtue has the same fixed standard." Beauty and truth, moreover, are one because, "after all, the most natural beauty in the world is honesty and moral truth. For all beauty is truth."¹⁷ Hence,

Inventing America; Peterson, *Thomas Jefferson and the New Nation*, 55; Chinard, "Jefferson among the Philosophers," *Ethics*, 53 (1942–43): 258; and Sheehan, *The Seeds of Extinction: Jeffersonian Philanthropy and the American Indian* (New York, 1973), 28–29. Jefferson's reliance on Moral Sense philosophy did not bar his consumption of Lockean thought. First of all, Moral Sense theorists themselves utilized Lockean psychology to some extent, and, second, for the realm of science Jefferson advocated the empirical methods of his trinity of heroes—Bacon, Locke, and Newton.

¹⁴ *Characteristics*, Shaftesbury's collected essays, went through eleven English editions by 1790, appeared in French translation in 1769, and was translated into German in 1776. His renown was well established in his own day; Montesquieu ranked him as one of "the four great poets" along with Plato, Malebranche, and Montaigne, and Herder accorded him the prestige of having "influenced the best heads of the eighteenth century." John Robertson, "Introduction," in Anthony Ashley Cooper, Earl of Shaftesbury, *Characteristics of Men, Manners, Opinions, Times, etc.* (1711), ed. Robertson, 1 (New York, 1900): xiv–xv, ix. Also see Stanley Grean, "Introduction," *Characteristics of Men, Manners, Morals, Opinions, Times* (1711), ed. John Robertson (Indianapolis, 1964), iv. And, for an indication of Shaftesbury's accessibility in American libraries, see David Lundberg and Henry F. May, "The Enlightened Reader in America, 1700 to 1813," *American Quarterly*, 28 (1976): 273.

¹⁵ For comparative texts of Shaftesbury's *Characteristics*, Jefferson's notebooks, and the Bill for Religious Freedom of 1786, see the Appendix, page 355, below. Jefferson's interest in *Characteristics* remained sufficiently strong for him to retain two additional editions of the work even after his bankruptcy forced him to sell a 1714 edition as part of the 1815 transaction with Congress; see E. Millicent Sowerby, *Catalogue of the Library of Thomas Jefferson*, 2 (Washington, 1953): 13, item no. 1258.

¹⁶ Establishing intellectual influences can, of course, be a treacherous undertaking. I am less interested in attempting such a task here, however, than in arguing for the shared intellectual orientation of Jefferson and Shaftesbury.

¹⁷ Shaftesbury, "Advice to an Author," in his *Characteristics*, ed. Robertson, 227, and "Freedom of Wit and Humour," *ibid.*, 94.

Shaftesbury shares with the ancient Greeks, whom he so often cited, the ideal that good conduct, moral action, leads to aesthetic harmony.

Shaftesbury's intent was to conjoin the notion of innate natural affections, which all sensible creatures (even animals) possess, with that of a pleasurable response akin to artistic enjoyment, of which only reflective beings are capable. In short, sensible creatures can attain goodness, but only reflective creatures can attain virtue. "In a creature capable of forming general notions of things," he explained, "not only the outward beings which offer themselves to the sense are the objects of the affection, but the very actions themselves, and the affections of pity, kindness, gratitude, and their contraries, being brought into the mind by reflection, become objects."¹⁸ The Moral Sense is thus a "second-order" natural affection, a reflected affection toward one's felt affections.¹⁹ In consequence, virtue is itself dependent upon an individual's reflective capabilities.

According to Shaftesbury, the faculties of imagination and reason especially aid an individual in attaining virtuous self-artistry, for in imagination "the appetites and desires are fabricated. . . . If I can stop the mischief here and prevent false coinage, I am safe." Furthermore, the Fine Arts themselves may act as a stimulus to imagination and virtue: "Even a romance, a poem, or a play may teach us; whilst the fabulous author leads us with such pleasure through the labyrinth of the affections, and interests us, whether we will or no, in the passions of his heroes or heroines." A fervent imagination is capable of exciting "Enthusiasm," a desirable feeling of "horror, delight, confusion, fear, admiration, or whatever passion belongs to it," in response to something "sublime," either a vision or a real object.²⁰ So powerful a tool is imagination that Shaftesbury likened it to "Divine Presence." Enthusiasm is not, however, a final objective, for imagination should act in concert with reason's discipline in order to attain life's ultimate goal: happiness through tranquillity. After a rush of sublime sensations, either horrific or delightful, body and mind seek tranquillity, the harmony inherent in nature. Pleasure in the passions of Enthusiasm, like a first gasp in the presence of great beauty, is followed by serenity of body and mind, an even greater pleasure.

Yet, in this model of virtue, the goal of serenity does not preclude righteous struggle, even warfare, on its behalf. Because the "interest of the world in general" is so wide in scope, the most direct path toward a universal social virtue may be *hidden or disguised by differences that exist between cultures*. Thus, war as a means to virtue, Shaftesbury observed, is at times necessary. Factionalism is most likely to occur within a society grown large—a vast, somewhat "unnatural" empire, in his estimation. War serves as a corrective by reinstating common affection, at least among partisans. "'Tis strange to imagine that war, which of all things appears the most savage, should be the passion of the most heroic spirits. But 'tis in war that

¹⁸ Shaftesbury, "Concerning Virtue or Merit," in his *Characteristics*, ed. Robertson, 251.

¹⁹ Gregory W. Trianosky has used this term to explicate Shaftesbury's concept of a Moral Sense; see Trianosky, "On the Obligation to Be Virtuous: Shaftesbury and the Question, Why Be Moral?" *Journal of the History of Philosophy*, 16 (1978): 296.

²⁰ Shaftesbury, "Advice to an Author," 207, "Freedom of Wit and Humour," 90, and "A Letter concerning Enthusiasm," in his *Characteristics*, ed. Robertson, 37–38.

the knot of fellowship is closest drawn.”²¹ In war, then, the exercise of affection can be restored and the direction toward virtue regained.

Testimony to Jefferson’s immersion in Shaftesbury’s mode of thinking—that is, a mentality that fuses art and morality—is abundant. Jefferson’s letter to Cosway spells out several features of the Moral Sense philosophy, including a list of the natural affections. To Cosway as well he poetically encapsulated in the Heart’s words the risk one takes to achieve happiness, acknowledging struggle as often-times necessary to the objective of tranquillity: “We have no rose without its thorn; no pleasure without alloy.” During the same year that the letter to Cosway was written, he more explicitly voiced a similar view to Ezra Stiles, in reference to Shays’s Rebellion: “If the happiness of the mass of the people can be secured at the expense of a little tempest now and then, or even of a little blood, it will be a precious purchase.”²²

A letter to his nephew, Peter Carr, in 1787 registers a more sober rendition of his views on morality than the version written to Cosway. Providing his nephew with educational guidelines, he discouraged Carr from attending lectures on Moral Philosophy and specifically distinguished between the roles of moral conduct and scientific calculation:

He who made us would have been a pitiful bungler if he had made the rules of our moral conduct a matter of science. For one man of science, there are thousands who are not. What would have become of them? Man was destined for society. His morality, therefore, was to be formed to this object. He was endowed with a sense of right and wrong, merely relative to this. This sense is as much a part of his nature, as the sense of hearing, seeing, feeling; it is the true foundation of morality. . . . It may be strengthened by exercise, as may any particular limb of the body. This sense is submitted, indeed, in some degree, to the guidance of reason; but it is small stock which is required for this.²³

Over the span of his adult life, Jefferson’s views on an innate Moral Sense remained fundamentally unchanged. In 1814 he wrote a complimentary letter to Thomas Law after reading Law’s *Second Thoughts on Instinctive Impulses*, which he found with “great satisfaction” to be in keeping with his “own creed on the foundation of morality in man.” In words reminiscent of those in the letter to Carr written twenty-seven years earlier, but carrying through an aesthetic metaphor more directly, he explained that to perform “good acts gives us pleasure” because “nature hath implanted in our breasts, a love of others, a sense of duty to them, a moral instinct, in short, which prompts us irresistibly to feel and to succor their distresses. . . . The Creator would indeed have been a bungling artist, had he intended man for a social animal, without planting in him social dispositions.”²⁴ In this letter, too, he distinguished between the Moral Sense and another one altogether distinct, “an innate sense of what we call *the beautiful*.” In brief, the Moral Sense delineates right and wrong, whereas the aesthetic sense addresses the domain of Fine Arts. But in

²¹ Shaftesbury, “Freedom of Wit and Humour,” 75–76.

²² Jefferson to Stiles, December 24, 1786, in *Jefferson Papers*, 10: 629.

²³ Jefferson to Carr, August 10, 1787, in *Jefferson Papers*, 12: 14–15.

²⁴ Jefferson to Law, June 13, 1814, in *The Writings of Thomas Jefferson*, 6: 348–51.

Jefferson's Classification Scheme as Printed in the 1815 Catalogue

BOOKS may be classed according to the faculties of the mind employed on them: these are—

I. MEMORY.

II. REASON.

III. IMAGINATION.

Which are applied respectively to—

I. HISTORY. II. PHILOSOPHY. III. FINE ARTS.

	Chapt.
I. HISTORY .	1
	2
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	4
	5
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	9
	10
	11
	12
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	14
	15
II. PHILOSOPHY	16
	17
	18
	19
	20
	21
	22
	23
	24

Mathematical	Pure	Arithmetic	25
		Geometry	26
		Mechanics	
		Statics	
		Dynamics	27
	Physico-Mathematical	Pneumatics	
		Phonics	
		Optics	
		Astronomy	28
		Geography	29
Architecture	Architecture	30	
	Gardening		
	Painting	31	
	Sculpture		
	Music	32	
Poetry	Epic	33	
	Romance	34	
	Pastorals		
	Odes	35	
	Elegies		
	Didactic	36	
	Tragedy	37	
	Comedy	38	
	Dialogue	39	
	Epistles		
Oratory	Logic		
	Rhetoric	40	
	Orations		
Criticism	Theory	41	
	Bibliography	42	
	Languages	43	
Polygraphical			44
III. FINE ARTS			
Authors who have written on various branches			

SOURCE: E. Millicent Sowerby, *Catalogue of the Library of Thomas Jefferson*, 1 (Washington, 1952).

Jefferson's view the two senses, the moral and the aesthetic, had to work together in order for human beings to attain virtue.

The views expressed to Cosway, Carr, and Law were not merely private sentiments; Jefferson stated them consistently in his public writings as well. In his "Rights of British America," he implored the king: "Open your breast, Sire, to liberal and expanded thought. . . . It behooves you, . . . to think and to act for yourself and your people. The great principles of right and wrong are legible to every reader; to pursue them requires not the aid of many counselors. The whole art of government consists in the art of being honest." The true art of government, he advised, would bring about "the restoration of that tranquillity for which all must wish" and would "establish fraternal love and harmony through the whole empire."²⁵

Other writings reveal an even more profound coupling of art and morals. Jefferson's letter of 1771 to Robert Skipwith expressly comments on the desired interplay between Fine Arts and conduct. Over half of the entries on the library list that Jefferson attached to his letter are classified as "Fine Arts," the type of literature that Jefferson designated as the proper arena for the faculty of imagination—a faculty that plays an important role in an aesthetics of virtue (see Chart 1). Much of the letter to Skipwith comprises a defense of the large number of "entertainments of fiction" on the list. Ever concerned with practice, Jefferson's apologia asserts their utility:

[E]very thing is useful which contributes to fix us in the principles and practice of virtue. When any signal act of charity or of gratitude, for instance, is presented either to our sight or imagination, we are deeply impressed with its beauty and feel a strong desire in ourselves of doing charitable and grateful acts also. On the contrary when we see or read of any atrocious deed, we are disgusted with its deformity and conceive an abhorrence of vice.

Thus, as Shaftesbury also noted, imagination—helping both to fabricate and to judge desires—encourages an individual to tread the most virtuous route. Describing how virtue is developed, Jefferson continued his letter:

Now every emotion of this kind is an exercise of our virtuous dispositions; and dispositions of the mind, like limbs of the body, acquire strength by exercise. . . . We never reflect whether the story we read be truth or fiction. If the painting be lively, and a tolerable picture of nature, we are thrown into a reverie. . . . I appeal to every reader of feeling and sentiment whether the fictitious murder of Duncan by Macbeth in Shakespeare does not excite in him as great horror of villainy, as the real one of Henry IV by Ravaillac as related by Davila?²⁶

Just as Shaftesbury had remarked that "even a romance, a poem, or a play may teach," so Jefferson noted that imagination enables one to experience vicariously the feelings of a literary hero or heroine and to "carry home to the mind every

²⁵ Jefferson, "A Summary View of the Rights of British America," in *Jefferson Papers*, 1: 134.

²⁶ Jefferson to Skipwith, August 3, 1771, in *Jefferson Papers*, 1: 76–77.

moral rule of life,” observing that such was his “idea of well-written Romance, of Tragedy, Comedy, and Epic Poetry.”²⁷

WHEREAS IN THE LETTER TO ROBERT SKIPWITH Jefferson touched primarily on imagination’s potency in creating virtue, in *Notes on the State of Virginia*, his one full-length work, he demonstrated through both form and content his conviction that the interaction of all mental faculties in concert with sentiment is absolutely vital to the cultivation of virtue. The title of the book is rather misleading; far from being just a compilation of data about the state of Virginia, *Notes* contains much implicit information about his theories of knowledge, government, and morality. And, in terms of Jefferson’s aesthetics, the work might more aptly be entitled “Notes on the State of Virtue.”

Notes on the State of Virginia originated from twenty-two “queries” by the secretary of the French legation in Philadelphia, the Marquis François de Barbé-Marbois. Jefferson’s rearrangement of the topics of the queries into twenty-three chapters shows how carefully he organized the structure of his work. Not only did Jefferson combine some questions, he also altered their order (see Chart 2). Thus, the chapters more closely follow the pattern of the classificatory system he employed in his own library, a system based upon his understanding of the faculties constituting the human mind—memory, reason, and imagination. Placing the appropriate parts of his library classification opposite his chapter headings for *Notes* reveals just how closely the structures of the two correspond and, more importantly, demonstrates the fundamental character of his aesthetics of virtue (see Chart 3).

Consistent with Jefferson’s aesthetics of virtue, *Notes on the State of Virginia* presents an evolutionary model of history. The work begins with a description of nature (Chapters 1–7), introduces humanity into nature (Chapters 8–11), captures the creation of a social order and revolutionary government (Chapters 12–13), describes the legal, educational, and economic institutions created by the new social order (Chapters 14–22), and concludes with memorials of the written word (Chapter 23). On its face, Chapter 23 does not rank as “Fine Arts,” but in that chapter Jefferson discussed the extant histories of the state primarily in terms of their style, thus revealing his concern for the rhetoric of history.²⁸ By beginning the

²⁷ *Ibid.*; and Shaftesbury, “Freedom of Wit and Humour,” 90. By contrast, it is striking to note John Locke’s vehement condemnation of “the art of rhetoric”: “Since wit and fancy finds easier entertainment in the world than dry truth and real knowledge, *figurative speeches* and allusions in language will hardly be admitted as an imperfection or *abuse* of it. I confess, in discourses where we seek rather pleasure and delight than information and improvement, such ornaments as are borrowed from them can scarce pass for faults. But yet, if we would speak of things as they are, we must allow that all the art of rhetoric, besides order and clearness, all the artificial and figurative application of words eloquence hath invented, are for nothing else but to insinuate wrong *ideas*, move the passions, and thereby mislead the judgment, and so indeed are perfect cheat; and therefore however laudable or allowable oratory may render them . . . they are certainly, in all discourses that pretend to inform or instruct, wholly to be avoided and, where truth and knowledge are concerned, cannot but be thought a great fault either of the language or person that makes use of them.” Locke, *An Essay concerning Human Understanding*, ed. John Yolton, 2 (New York, 1964): Bk. 3, chap. 10.

²⁸ For example, Jefferson labeled the style of Captain John Smith’s history “barbarous and uncouth,” and he claimed that William Stith’s work is “inelegant,” for Stith had “no taste in style”; Jefferson, *Notes on the State of*

CHART 2
Comparison of Marbois's Queries and Jefferson's Chapters

MARBOIS'S ORIGINAL QUERIES	JEFFERSON'S ARRANGEMENT
1. The Charters of your state.	1. Boundaries of Virginia [3]
2. The present Constitution.	2. Rivers [6]
3. An exact description of its limits and boundaries.	3. Sea-ports [13]
4. The Memoirs published in its name, in the time of its being a Colony and the pamphlets relating to its interior or exterior affairs, present or ancient.	4. Mountains [6]
5. The History of the State.	5. Cascades [6]
6. A notice of the Counties Cities Townships Villages Rivers Rivulets and how far they are navigable. Cascades Caverns Mountains Productions Trees Plants Fruits and other natural Riches.	6. Productions Mineral, Vegetable and Animal [6, 20]
7. The number of its Inhabitants	7. Climate
8. The different Religions received in that State.	8. Population [7]
9. The Colleges and Public establishments. The Roads, Buildings &c.	9. Military Force [18]
10. The Administration of Justice and a description of the Laws.	10. Marine Force [19]
11. The particular Customs and manners that may happen to be received in that State.	11. Aborigines [22]
12. The present State of Manufactures Commerce interior and exterior Trade.	12. Counties and Towns [6]
13. A notice of the best Sea Ports of the State and how big are the vessels they can receive.	13. Constitution [1, 2]
14. A notice of the commercial productions particular to that State and of those objects which the Inhabitants are obliged to get from Europe and from other parts of the World.	14. Laws [1, 2]
15. The weight measures and the currency of the hard money. Some details relating to the exchange with Europe.	15. Colleges, Buildings, and Roads [9]
16. The public income and expenses.	16. Proceedings as to Tories [17]
17. The measures taken with regard of the Estates and Possessions of the Rebels commonly called Tories.	17. Religion [8]
18. The condition of the Regular Troops and the Militia and their pay.	18. Manners [11]
19. The marine and Navigation.	19. Manufactures [12, 14]
20. A notice of the Mines and other Subterranean riches.	20. Subjects of Commerce [12, 14]
21. Some samples of these Mines and of the extraordinary Stones. In short a notice of all what can increase the progress of human Knowledge.	21. Weights, Measures, and Money [15]
22. A description of the Indians established in the State before the European Settlements and of those who are still remaining. An indication of the Indian Monuments discovered in that State.	22. Public Revenue and Expenses [16]
	23. Histories, Memorials, and State-Papers [4,5]

SOURCES: Marbois' Queries concerning Virginia, in [Jefferson] *The Papers of Thomas Jefferson*, ed. Julian P. Boyd, 4 (Princeton, 1951): 166–67; and Jefferson, *Notes on the State of Virginia* (Paris, 1787).

CHART 3

Chapter Headings Classed by Faculties of Mind and Corresponding Applications

FACULTIES OF THE MIND		CHAPTER HEADINGS
I. History (Memory)	Natural	1. Boundaries of Virginia
		2. Rivers
		3. Sea-ports
		4. Mountains
		5. Cascades
		6. Productions Mineral, Vegetable and Animal
	Civil	7. Climate
		8. Population
		9. Military Force
		10. Marine Force
		11. Aborigines
		12. Counties and Towns
II. Philosophy (Reason)	Ethics	13. Constitution
		14. Laws
		15. Colleges, Buildings, and Roads
		16. Proceedings as to Tories
		17. Religion
	Moral	18. Manners
		19. Manufactures
		20. Subjects of Commerce
		21. Weights, Measures, and Money
		22. Public Revenue and Expenses
III. Fine Arts (Imagination)	Jurisprudence	23. Histories, Memorials, and State-Papers

work with features of memory, proceeding to those of reason, and ending with those of imagination (as well as by producing a work that is the product of all three faculties working together), Jefferson incorporated the mental faculties and their interaction necessary to his aesthetics of virtue. Finally, the Socratic structure of *Notes* suggests its ultimate aim—to give knowledge about the practice of virtue to its questioning readers.

In two notable passages from *Notes on the State of Virginia*, often anthologized, Jefferson specifically laid out the interaction of memory, reason, imagination, and sentiment, thereby more fully disclosing his aesthetics. Describing an observer's response to nature's and, more significantly, to Virginia's sublimity, he depicted an incomparable landscape. The first description occurs in Chapter 4, "Mountains." Infusing the passage with a dynamic similar to Shaftesbury's Enthusiasm-tranquillity model, Jefferson enticed his readers into sharing his experience of nature's power and ultimate harmony:

The passage of the Patowmac through the Blue Ridge is perhaps one of the most stupendous scenes in nature. You stand on a very high point of land. On your right comes up the Shenandoah, having ranged along the foot of mountain an hundred miles to seek a vent. On your left approaches the Patowmac, in quest of a passage also. In the moment

Virginia, 177. Two excellent analyses of Jefferson's writings that, unfortunately, I did not come across until after this essay had reached the proof stage are William J. Sheick, "Chaos and Imaginative Order in Thomas Jefferson's *Notes on the State of Virginia*," and Stephen D. Cox, "The Literary Aesthetic of Thomas Jefferson," both in J. A. Leo LeMay, ed., *Essays in Early Virginia* (New York, 1977).

of their junction they rush together against the mountain, rend it asunder, and pass off to the sea. The first glance of this scene hurries our senses into the opinion, that this earth has been created in time. . . .

In this passage, the observer and nature are equally crucial to the landscape; without the beholder, the setting is incomplete, for humanity is an integral part of nature's harmony. Reciprocally, humanity is incomplete without nature, for nature provides the vision that thrusts its observers into a passionate response. Jefferson continued his description, depicting the outcome of the violent junction of the two rivers:

But the distant finishing which nature has given to the picture is of a very different character. It is a true contrast to the foreground. It is as placid and delightful, as that is wild and tremendous. For the mountains being cloven asunder, she presents to your eye, through the cleft, a small catch of blue horizon, at an infinite distance in the plain country, inviting you, as it were, from the riot and tumult roaring around, to pass through the breach and participate in the calm below.²⁹

As the aesthetic eye for his readers, Jefferson became their intermediary, providing a memorial of his own experience of nature's artwork so that his readers too might be exhilarated by the elevation of their senses, opine nature's creation, and then imagine—or, perhaps, co-create—the tranquillity promised in the distant horizon. The aesthetic vision itself is thus fused with a call for participation.

A second portrait of nature corresponds to this same paradigm of rushing senses followed by calm. To describe the Natural Bridge flanking his land near Monticello, Jefferson began with statistics to provide the reader with a scientific measure of nature, but he then left science behind to relate the rapturous emotions viewing the scene evokes:

The *Natural Bridge*, the most sublime of Nature's works, . . . must not be pretermitted. . . . Though the sides of this bridge are provided in some parts with a parapet of fixed rocks, yet few men have resolution to walk to them and look over into the abyss. You involuntarily fall on your hands and feet, creep to the parapet and peep over it. Looking down from this height about a minute, gave me a violent head ach.

Although the strength of his response to the scene was overwhelming when viewed from atop the bridge, he discovered that

descending then to the valley below, the sensation becomes delightful in the extreme. It is impossible for the emotions, arising from the sublime, to be felt beyond what they are here: so beautiful an arch, so elevated, so light, and springing, as it were, up to heaven, the rapture of the Spectator is really indescribable!³⁰

Jefferson's use of the word "sublime" twice within this short passage bespeaks the rapturous state of body-mind that Shaftesbury lauded.³¹ But sublimity is not the

²⁹ Jefferson, *Notes on the State of Virginia*, 19.

³⁰ *Ibid.*, 24–25.

³¹ Wills's discussion of Jefferson's passage about the Natural Bridge, though most illuminating in its gloss on "sublimity," is nevertheless frustrating in its struggle to maintain the thesis of Jefferson's scientific propensity.

final objective here either, only the means by which a sense of harmony is achieved. From the Latin *sublimare* ("to raise"), the word's etymology attests movement or change in a positive direction. The Natural Bridge therefore becomes emblematic: happiness cannot be achieved without intermediary stages, sometimes horrific, as in the violent headache Jefferson reported; yet the bridge signals the passage to a better future, and Jefferson as author became a bridge-building artist, exercising the fullness of his mental faculties to bring revolutionary colonists to virtuous freedom and independence.

An entirely different sort of description also appears in *Notes on the State of Virginia*, underscoring all the more Jefferson's insistence on the interaction of moral sentiment with all of the mental faculties in the realization of an aesthetics of virtue. Indeed, with such a model in mind, Jefferson's views on both the "equality" and "inferiority" of blacks become less paradoxical. His belief in every human being's innate Moral Sense led him to accept the equality and natural rights of all and, therefore, to call for the emancipation of slaves. Blacks have no "depravity of the Moral Sense," he stated emphatically.³² And yet, he argued, there is a difference "fixed in nature"—namely, that whites possess the greater "share of beauty in the two races" and that this difference signals the need for continued separation of the races.³³ Blacks lack beauty, he asserted, having instead that "eternal monotony," that "immovable veil of black which covers all the emotions." To one for whom emotions carried significant weight in cultivating virtue, such cloaking was a serious matter. But his speculations on the differences in mental faculties is even more crucial. "Comparing them by their faculties of memory, reason, and imagination," he wrote, "in memory they are equal to whites; in reason much inferior, as I think one could scarcely be found capable of tracing and comprehending the investigation of Euclid; and . . . in imagination they are dull, tasteless, and anomalous." By way of contrast, he assigned to native Americans a "reason and sentiment strong" and an "imagination glowing and elevated." But nowhere, he insisted, had he found a black who "had uttered a thought above the level of plain narration"; never had he seen "even an elementary trait of painting or sculpture" or poetry. As for love, theirs is "ardent," he admitted, "but it kindles the senses only, not the

Despite admitting that Jefferson's observations of the Natural Bridge proceed from Science to Nature to Art (in contrast to those of Chastellux, whose description Wills quoted and whose order is Nature to Art to Science), Wills has argued that for Jefferson sublimity was essentially a scientific observation and defended his conclusion by asserting rather boldly that, "when men of Jefferson's generation talked of sublimity . . . they were bound to think of one man's theory"—Edmund Burke's *Philosophical Enquiry into the Origins of Our Ideas of the Sublime and Beautiful* (1757). Burke, Wills insisted, was "rigorous in his logic and reductionism" (and, therefore, so the faulty syllogism goes, Jefferson was too). Wills, *Inventing America*, 259–72. Even a quick glance through the *Oxford English Dictionary* indicates that sublimity was a word well entrenched in the vocabulary of literature, philosophy, and architecture during the seventeenth and eighteenth centuries. In short, neither is it surprising that Jefferson used the word "sublimity" as often as he did nor is it necessary to insist that he did so with Burke in mind. What is essential about this passage is that it moves from Science to Nature to Art.

³² Jefferson, *Notes on the State of Virginia*, 142.

³³ "It is not against experience to suppose," Jefferson wrote, "that different species of the same genus, or varieties of the same species, may possess different qualifications. Will not a lover of natural history then, one who views the gradations in all the races of animals with the eye of philosophy, excuse an effort to keep those in the department of man as distinct as nature has formed them?" *Notes on the State of Virginia*, 143.

imagination." It is not their enslavement that accounts for these deficiencies, he added, for among the Romans "their slaves were often their rarest artists."³⁴

Doubtless Jefferson's racism prompted him to distinguish among whites, blacks, and native Americans, a prejudice that, in turn, blunted his ability to assess black artistry. But the rationale for his racism is perfectly consonant with his concept of virtue as the confluence of sentiment, memory, reason, and imagination. Because he saw blacks as inferior in reason and as dull and tasteless in imagination, Jefferson concluded that they constituted an obstacle to the creation of virtue, clearly a danger for the developing Moral Sense of the newly independent white Americans. His solution entailed gauging what he perceived to be the common good for both whites and blacks and proposing what he considered to be reasonable policies: education, emancipation, and recolonization of the former slaves. Viewing slaves as members of a "distinct race" lacking natural harmony with the white race and believing that slavery necessarily bred hatred among slaves for their captors, he proposed to resettle them in colonies and aid them in building their own society: "sending them out with arms, implements of household and of the handicraft arts, seeds, pairs of the useful domestic animals, &c. to declare them a free and independent people, and extend to them our alliance and protection, till they shall have acquired strength."³⁵ Jefferson felt that to free the slaves before educating them would be a grave injustice to them but that to retain them in America would be a graver injustice, disrupting the social harmony of white Americans, upon whom his loyalties and artistic tastes were focused.

SOCIAL AND PERSONAL HARMONY remained Jefferson's twin goals, the dual aims of his aesthetics of virtue. Typically, he simply called such harmony "happiness." No doubt his belief is most conspicuously stated in the claim of the Declaration of Independence for the pursuit of happiness as an unalienable right. Yet an assertion of happiness as life's objective was not new for Jefferson in 1776. As early as 1763 he had expressed similar views to his friend John Page: "Perfect happiness I believe was never intended by the deity to be the lot of any one of his creatures in this world; but that he has very much put in our power the nearness of our approaches to it, is what I as steadfastly believe." On many occasions after the Declaration he reiterated this view. In 1782, for instance, he wrote to James Monroe that "the giver of life . . . gave it for happiness and not for wretchedness." And, in 1798, he characterized himself as one "who considers social harmony as the first of human felicities, and the happiest moments those which are given to the expressions of the heart," thus explicitly linking happiness, harmony, and heart.³⁶

Jefferson's self-acknowledged Epicureanism also reflects his view that happiness, a pleasurable harmony rooted in virtue, is the proper "aim of life." In 1807 he

³⁴ Jefferson, *Notes on the State of Virginia*, 138–42. ³⁵ *Ibid.*, 138.

³⁶ Jefferson to Page, July 15, 1763, in *Jefferson Papers*, 1: 10; Jefferson to Monroe, May 20, 1782, *ibid.*, 6: 184; and Jefferson to —, 1798, as quoted in John P. Foley, ed., *The Jeffersonian Encyclopedia*, 1 (New York, 1900): 400.

CHART 4
Jefferson's Syllabus of Epicureanism

SYLLABUS OF THE DOCTRINES OF EPICURUS

Physical.—The Universe eternal.
Its parts, great and small, interchangeable.
Matter and Void alone.
Motion inherent in matter which is weighty and declining.
Eternal circulation of the elements of bodies.
Gods, an order of beings next superior to man, enjoying
in their sphere, their own felicities; but not meddling
with the concerns of the scale of beings below them.

Moral.—Happiness the aim of life.
Virtue the foundation of happiness.
Utility the test of virtue.
Pleasure active and In-do-lent.
In-do-lence is the absence of pain, the true felicity.
Active, consists in agreeable motion; it is not happiness,
but the means to produce it.
Thus the absence of hunger is an article of felicity;
eating the means to obtain it.
The *summum bonum* is to be not pained in body, nor troubled
in mind.
i.e. In-do-lence of body, tranquillity of mind.
To procure tranquillity of mind we must avoid desire and
fear, the two principal diseases of the mind.
Man is a free agent.
Virtue consists in 1. Prudence. 2. Temperance.
3. Fortitude. 4. Justice.
To which are opposed, 1. Folly. 2. Desire. 3. Fear.
4. Deceit.

SOURCE: Thomas Jefferson to William Short, October 31, 1819, in [Jefferson] *The Writings of Thomas Jefferson*, ed. H. A. Washington, 7 (Washington, 1854): 138.

confirmed his allegiance to Epicureanism during an after-dinner conversation with John Quincy Adams. "Mr. Jefferson," Adams recorded later in his diary, "said that the *Epicurean* philosophy came nearest to the truth, in his opinion, of any ancient system of philosophy, but that it has been misunderstood and misrepresented." In a letter to William Short in 1819, Jefferson observed that Epicureanism contained "everything rational in moral philosophy which Greece and Rome have left us" and acknowledged to Short, "As you say of yourself, I too am an Epicurian."³⁷ To the letter he attached a copy of a syllabus of Epicureanism that he had drawn up "some twenty years ago" and written in "lapidary style" (see Chart 4).

But, if Epicurus had stressed the rational element of virtue, then to the wisdom

³⁷Adams, *Memoirs*, ed. Charles Francis Adams, 1 (Philadelphia, 1874): 472; and Jefferson to Short, October 31, 1819, in *The Writings of Thomas Jefferson*, 7: 138.

of rationality Jefferson added the wisdom of sentiment, thus conjoining head and heart in dialectical unity. Ironically, the passage that most fully echoes his aesthetics of virtue is one that was stricken from the Declaration, a passage pronouncing the wrongdoings of the British people themselves toward the colonists: "and when occasions have been given them, by the regular course of their laws, of removing from their councils the disturbers of our harmony, they have, by their free election, re-established them in power." Entitled to social harmony by God's grand design, the colonists must perforce struggle to regain it against "these unfeeling brethren" who "have given the last stab to agonizing affection."³⁸ Although Carl Becker viewed these deletions as a stylistic improvement, Jefferson himself was deeply troubled by them and regarded the excision as a "depredation." Indeed, in its original form, this section serves as the culmination of the Declaration's grievances. Becker deemed Jefferson's "felicity of expression" as akin to "resting one's weight on something fragile."³⁹ In contrast to Becker's reservations, Garry Wills has declared that happiness carried for Jefferson a scientific precision, a measurability.⁴⁰ But it is not necessary either to dismiss as flowery rhetoric or to legitimize as scientific specificity his exultations about happiness. In an aesthetics of virtue, happiness and harmony emerge as nothing less than the imperatives of God's and nature's artistry, an artistry certainly not to be ignored by humankind.

In the emergence of *homo aestheticus*, Jefferson attended to the role to be played by individual artists as well as to God's grand design for the species. On this individual level, Monticello must be reckoned as his contribution to America's burgeoning arts. In the pages of *Notes on the State of Virginia*, he lamented that the "genius of architecture seems to have shed its maledictions over this land"; and the many years he spent designing and remodeling Monticello were, perhaps, his own rite of exorcism. Indeed, Monticello represents a fitting icon of his aesthetics of virtue. High atop the leveled plateau of a mountain, Monticello stands serenely overlooking the wilderness landscape that envelops it and provides a striking contrast to its classical architecture. To reach Monticello, visitors must pass through the sublimity of the "workhouse of nature" before entering the tranquillity and harmony of the human-made edifice. And, once there, the ingress through nature remains ever visible as a symbol of what humanity, working in concert with nature, may achieve.

Although Monticello attests most overtly to Jefferson's artistic accomplishments, he was no less an artist of language and society. Yet, despite his subsequent immortalization as a nearly mythical American hero, Jefferson's voice was often swamped by a sea of noise carrying views of art and morality far different from his own. The dialectical quality that characterized his aesthetics of virtue was overwhelmed by nineteenth-century dualities that dichotomized humanity and nature, sentiment and rationality, art and morality.⁴¹ Jefferson's talent for uniting aesthetics

³⁸ Jefferson, Draft of the Declaration of Independence, as quoted in Carl Becker, *The Declaration of Independence* (1922; reprint edn., New York, 1958), 182–83.

³⁹ Becker, *The Declaration of Independence*, 208–12, 218.

⁴⁰ Wills, *Inventing America*, 164, 240–55.

⁴¹ This is not to deny Jefferson's own dualisms. He did not, for instance, challenge a male-dominated political system. Sex-role divisions already characteristic of his day were both the product of and a reinforcement for a

and morality has thus remained all too rare. It is, therefore, hardly surprising that the course of American development has left largely unfulfilled the sanguine view that he expressed about his country's future in *Notes on the State of Virginia*: "America, though but a child of yesterday, has already given hopeful proofs of genius."⁴²

APPENDIX

In his notebooks, Jefferson recorded the following passage almost word for word from Shaftesbury's "A Letter concerning Enthusiasm" (italics added):

Not only the visionaries and enthusiasts of all kinds were tolerated, . . . by the ancients; but, on the other side, philosophy had *as free a course*, and was permitted as a balance against superstition. And whilst some sects, such as the Pythagorean and latter Platonic, joined in with the superstition and enthusiasm of the times; the Epicurean, the Academic, and others, *were allowed to use all the force of wit and raillery* against it. And thus matters were happily balanced; *reason had fair play*; learning and science flourished. Wonderful was the harmony and temper which arose from all these contrarieties. Thus superstition and enthusiasm were mildly treated, and being let alone they never raged to that degree as to occasion bloodshed, wars, persecutions, and devastation in the world. *But a new sort of policy, which extends itself to another world and considers the future lives and happiness of man rather than the present, has made us leap the bounds of natural humanity*; and out of a supernatural charity has taught us the way of plaguing one another most devoutly. It has raised an antipathy which no temporal interest could ever do; and entailed upon us a mutual hatred to all eternity. And now *uniformity in opinion* (a hopeful project!) is looked on as the only expedient against this evil. The saving of souls is now the heroic passion of exalted spirits; and *is become in a manner the chief care of the magistrate, and the very end of government itself*.

If the magistracy should vouchsafe to interpose thus much in other sciences, I am afraid we should have as bad logic, as bad mathematics, and in every kind as bad philosophy, as we often have divinity, in countries where a precise orthodoxy is settled by law.

To this passage Jefferson added thoughts of his own that underscore his agreement with Shaftesbury:

[suppose the state should take into head that there should be an uniformity of countenance. men would be obliged to put an artificial bump or swelling here, a patch there, &c. but this would be merely hypocritical.] or if the alternative was given of wearing a mask, 99/100ths must immediately mask. would this add to the beauty of nature? why otherwise in opinions.] [in the middle ages of Xty. opposition to the state opns. was hushed. the consequence was, Xty. became loaded with all the Romish follies. nothing but free argument, raillery, & even ridicule will preserve the purity of religion.]

dualism that was further exacerbated by economic forces that pushed men and women into separate public and private spheres. With this division, rationality came to be seen as exclusively a masculine domain, sentimentality a feminine one.

⁴² Jefferson, *Notes on the State of Virginia*, 65.

Passages from the Bill for Religious Freedom of 1786 correspond directly to those passages that Jefferson copied from Shaftesbury and his own notes on the same letter:

Well aware that . . . Almighty God hath created the mind free, . . . that all attempts to influence it by temporal punishments . . . tend only to beget habits of hypocrisy . . . , that the impious presumption of legislators and rulers, . . . have assumed dominion over the faith of others, setting up their own opinions and modes of thinking as the only true and infallible, . . . that our civil rights have no dependence on our religious opinions, any more than our opinions in physics and geometry; . . . that the opinions of men are not the object of civil government, . . . and, finally, that truth is great and will prevail if left to herself; that she is the proper and sufficient antagonist to error, and has nothing to fear from the conflict unless by human interposition disarmed of her natural weapons, free argument and debate; errors ceasing to be dangerous when it is permitted freely to contradict them.*

* Jefferson, *Notebooks*, in *Jefferson Papers*, 1: 548–49.

Jefferson also recorded at this time passages from Locke, but, as Merrill Peterson has noted, the final bill rejects “Lockean toleration as insufficient, since it implied an official or preferred religion”; Peterson, “Introduction,” in Peterson, ed., *The Portable Thomas Jefferson* (New York, 1975), xx.

Comment la Politique Vint aux Paysans: A Second Look at Peasant Politicization

EUGEN WEBER

VILLAGES, THE STUDENT OF A JAPANESE HAMLET recently remarked, are rather like a stage where most of the action goes on with the curtain down.¹ Historians prefer the curtain up and the lights on. They have tended to leave the crabbed evidence of the village to more frugal colleagues in ethnology and political science. As a result, students of Asia, Africa, and Latin America know more—or have more publications to read—about the peasantries of these continents than do the historians of peasant life in modern Europe. And rural studies in European history are closer to center stage for earlier centuries (and for the twentieth, where ethnologists and political scientists set the lead) than for the nineteenth. In French history, in which so much original work has been done, scholars have concentrated on the period before 1800. André Siegfried's *Tableau politique de la France de l'Ouest sous la Troisième République* (1913) remained a lonely monument pretty much into the 1960s, when several impressive publications appeared.²

The 1970s welcomed richer fare. Following Philippe Vigier, a number of scholars came to see the brief tenure of the Second Republic as a crucial time of rural politicization, when, in Charles Tilly's words, "the national state won a durable victory over local power-holders and traditional particularisms." The best work of this school, Ted W. Margadant's *French Peasants in Revolt* (1979), concludes that by 1851 significant sections of the peasantry—"increasingly integrated into a national economy, an urban society, and a centralized political system"—had learned to consider political issues that were more than local and to participate in national political activities: witness the peasant insurrection of December 1851.³

I owe thanks to the Rockefeller Foundation, in whose Villa Serbelloni, at Bellagio, the first draft of this article was written in 1980.

¹ Robert J. Smith, *Kurusu: The Price of Progress in a Japanese Village, 1951–1975* (Stanford, 1978), 233.

² Georges Dupeux, *Aspects de l'histoire sociale et politique du Loir-et-Cher, 1848–1914* (Paris, 1962); Philippe Vigier, *La Seconde République dans la région alpine: Étude politique et sociale*, 2 vols. (Paris, 1963); Gordon Wright, *Rural Revolution in France: The Peasantry in the Twentieth Century* (Stanford, 1964); and Maurice Agulhon, *La République au village: Les Populations du Var de la Révolution à la Seconde République* (Paris, 1970).

³ Tilly, "How Protest Modernized in France, 1845–1955," in William O. Aydelotte et al., eds., *The Dimensions of Quantitative Research in History* (Princeton, 1972), 226; and Margadant, *French Peasants in Revolt: The Insurrection of 1851* (Princeton, 1980), 338. Other works have situated this process of economic integration and political participation in a later period. Like Siegfried, who paid great attention to the evolution of economic and social structures, they have recognized the wide diversity of rural experience but have seen the crucial changes beginning to occur in the late 1870s and the 1880s. See Pierre Barral, *Les Agrariens français de Méline à Pisani* (Paris, 1968); Alain Corbin, *Archaisme et modernité en Limousin au XIX^e*

Some years ago I wrote a book about how the peasants in certain parts of nineteenth-century France, still largely indifferent to the national state and to the urban-led world around them, were gradually lured into that “modern” orbit; they were, in other words, integrated into systems of values and activities that had long remained, or appeared to them, irrelevant.⁴ I argued then that personal considerations reigned supreme over local politics, in which ideologies played very minor roles, but that, by the turn of the century, new conflicts and solidarities—now on a national scale—had penetrated, new possibilities had been apprehended in, regions indifferent to them a generation or two before. What I did not sufficiently do was to describe the personal and interpersonal dimensions of a process, predictable enough in itself though scarcely traced before, by which the language, gestures, and perceptions of national politics penetrated the countryside.

Other authors have lately considered this question in greater or lesser detail. Uncertain of the extent to which politicization had advanced, albeit haltingly, in the backward Limousin by 1880, Alain Corbin has seen it in the guise of a left-wing tradition that reflects economic quandaries and social tensions at the grass roots. Tony Judt, writing about the more urbanized villages of southeastern France at the turn of the century, has attributed the shift of some villagers toward socialism to practical experience and economic difficulties. The novelty in the Var of the 1880s and 1890s was that more peasants now glimpsed the solution to their local difficulties in national “political” action. Other historians—Pierre Barral, Maurice Agulhon, and Siegfried—have also equated, each in his own way, the electoral advances of the Left, whatever its label, with politicization. Yet such a view, however justified, still seems to beg the question. Politicization is not about moves to the Left (or Right) but about an awareness that alternatives exist, that choices are possible, that “political” activities are not about irrelevant abstractions but are closely related to social and economic concerns that are local, personal, and immediate. So voting Left does not, per se, prove politicization. It can just as easily reflect the survival of social cohesions and of traditional views rooted in the past. And the enduring political traditions—Red, White, or Blue—that have become established in regions like the Vendée, Var, or Limousin argue even more convincingly for persistent immobilities than for socioeconomic—and, hence, political—evolution.

Alain Corbin remarked that, at least until 1876, electoral results in those areas of the Limousin that were more exposed to national currents (the villages that, for example, sent migrant masons to Paris and other large cities) turned out “slightly less unfavorable to the Right,”⁵ which provides an indication, one would think, of the greater likelihood of individual self-affirmation in places where the traditional pressures and limitations of rural life had eased. Greater participation in market economy, more opportunity for individual enterprise and enhanced awareness that

siècle, 1845–1880, 2 vols. (Paris, 1975); Theodore Zeldin, *France, 1848–1945*, 2 vols. (Oxford, 1973–77); and Tony Judt, *Socialism in Provence, 1871–1914: A Study in the Origins of the Modern French Left* (Cambridge, 1979). Even Maurice Agulhon seems to have endorsed this view in the chapters he contributed to Georges Duby and Armand Wallon, eds., *Histoire de la France rurale* (Paris, 1976–77); see *ibid.*, volume 3: *Apogée et crise de la civilisation paysanne, 1789–1914*, pt. 1: chaps. 1, 3, 5, pt. 2: chaps. 4–5, and pt. 3: chaps. 4–5.

⁴ Weber, *Peasants into Frenchmen: The Modernization of Rural France, 1870–1914* (Stanford, 1976).

⁵ Corbin, *Archaisme et modernité en Limousin*, 2: 1004.

this opportunity exists, relaxation of social constraints, and the appearance of alternatives—all work *not* to politicize but to create conditions in which politicization can take place. The key question, then, is whether we can mark the passage from what Corbin has called the “territorial” (localistic) stance to a broader vision of national society and national affairs. To do this properly, we need access to the records of some thirty-six thousand communes. But can we at least determine a few major factors and stages of this passage? Can we date a few turning points? And can we go beyond the statistical to the personal and the local?

I WANT TO EMPHASIZE THE PERSONAL FACTOR. Occurring at the level of village, hamlet, and farm, the political acculturation of the peasantry should also be seen as a multitude of individual evolutions in which personalities played a part. That part is no less crucial for being so hard to trace. I do not mean to deny the significance of other factors but to suggest the more modest context within which they evoked interpretation and response at the individual level. I shall try to suggest—after some fresh forays among the documents—just how national politics initially were grafted onto local issues, which were dependent upon relationships of clans or kin or friends with their sociabilities and their feuds.⁶ Narrow interests could open the way to broader perspectives, but only in the service of individual or locality.

But first we must consider the problem of chronology, easy to lose from sight since the various localities and regions of France were at different stages of political evolution at any given time. Very roughly, political rivalries among the notables left the populace unengaged (except for local aspects) at least into the 1860s. Even so, one may well ask how much ideology and what Alexis de Tocqueville called “politics properly speaking” entered into the electoral activities of the enfranchised. “Very little, infinitely little,” the deputy of Valognes assured his friend, Gustave de Beaumont, in 1842.⁷ This only started to change in the 1860s, as factional conflicts were nationalized by a growing awareness of national politics (war, military service, tariff measures) impinging on local life. Beginning in the later 1860s and culminating in 1877, freer (or at least livelier) elections and a greater variety of candidates introduced real competition for the popular vote. Finally, during the 1880s, the peasants learned to use the ballot to improve their economic position, and those who competed for their vote learned to promise concrete returns, on whose delivery they would be judged. At that point, alignments based on interest (such as those for which Judt has argued in the Var) came into their own, and the process of acculturation appears to have run its course.

Reality was, of course, far less systematic than all of this might suggest. As late as 1913, Siegfried recognized that many Frenchmen did not regard as essential “the great problems on which the country divides and by which it orders the direction of

⁶ Judt has suggested that family feuds flourish wherever political and social variety is slight; *Socialism in Provence, 1871–1914*, 206–07. But it could be that, without such conflicts, political variety would lack the opportunity to appear.

⁷ Alexis de Tocqueville to Beaumont, June 21, 1842, in Tocqueville, *Correspondance d’Alexis de Tocqueville et de Gustave de Beaumont*, ed. André Jardin, volume 8 of Tocqueville, *Oeuvres complètes*, ed. J.-P. Mayer, 3 pts. (Paris, 1967), pt. 1: 459.

its destinies.” Rather, they were “moved by reasons that are local, special, and often incomprehensible to an outsider.”⁸ In the provinces, politics were a function of private relations. Since 1848, at least, publicists, ideologues, and government servants had sought to shift politics from this “trivial” plane. They would have liked to find questions of principle, not merely the petty warfare based on local animosities between hamlet Montagues and village Capulets. In fact, only too often, “questions of principle [gave] way to questions of personalities.”⁹ What was said of Die, in the mountains of the Drôme, was true of many another small town in 1865: “Idleness and pride have long since rent the upper bourgeoisie and divided the region into coteries, whose leaders become the representatives of a political party in order to give themselves more importance. But convictions play only a secondary role in their choice, and the flag they flourish always conceals personal animosities.”¹⁰

Twelve years later, in 1877, the Third Republic faced its first great trial: the president, Marshall MacMahon, replaced the Republican government with a minority cabinet, dissolved parliament, and held new elections in an atmosphere redolent of 1851.¹¹ At Saint-Julien-de-Civry (Saône-et-Loire) the schoolmaster described the village caught in the new political crisis in terms similar to those used to depict Die. The village was torn, but were there well-defined political parties? Certainly, “if one considered only the labels” that defined the hostile camps; certainly not, if one looked for other differences between them, all of which lay “in hostilities, hatreds, rivalries, profound jealousies.” This dichotomy between the labels and their substance continued to be true throughout much of rural France at least until 1914.¹²

Increasingly after 1830, national administrative intervention altered local politics but initially did little to elevate them above narrow social rivalries, which provided the highway for the eventual infiltration of national politics into the provinces. First reaching the “intelligent” or “enlightened” minorities and finally the masses, national alignments came to define local animosities. The periodic reports of departmental prefects and attorneys-general (*procureurs-généraux*) show how personal and local rivalries with no political overtones provided a focus (or inspiration)

⁸ Siegfried, *Tableau politique de la France de l'Ouest sous la Troisième République* (Paris, 1913), 96. Laurence Wylie and his co-authors have made a similar point: “Party labels are not meaningless: each party and name have an ideological significance that is well understood in Chazeaux. But it is a significance that must be understood in local and to some extent historical terms”; Wylie, ed., *Chazeaux: A Village in Anjou* (Cambridge, Mass., 1966), 267.

⁹ Procureur-Général, Roanne, June 18, 1857, Archives Nationales, Paris [hereafter, AN], BB 18 1567; and Sous-préfet, Parthenay, to Préfet, Deux-Sèvres, June 12, 1863, as quoted in Bernard LeClère, *Stephen Liégeard (1830–1925): Essai de réhabilitation du Sous-Préfet aux champs* (Paris, 1968), 73. All subsequent citations of the BB series refer to the reports of attorneys-general attached to the great regional courts of appeal and will be cited simply by the court’s regional name.

¹⁰ Grenoble, August 9, 1865, AN, BB 1717. Such frictions could be of the most futile kind; see Henri Prost, *Charles Dumont et le Jura* (Paris, 1964), 33.

¹¹ Indeed, the tone of the election was set by the marshal’s Bonapartist supporters, like the interior minister, Oscar Bardi de Fourtou, eager to prove himself another Morny. The Bonapartist-style electoral propaganda probably succeeded in mobilizing hitherto indifferent areas even more than Republican efforts did. In some departments, 12 percent more voters participated in 1877 than had turned out for the elections of 1876.

¹² Mona Ozouf, *La Classe ininterrompue* (Paris, 1979), 359.

for political opposition and how men with no intention of opposing the regime were gradually cast in this unintended role.¹³

This transformation is especially evident in elections for municipal offices and for the departmental Conseil Général. Frequently apolitical in character, these elections were the obvious proving grounds for men with wider political aspirations; as such, they cannot be ignored when considering the general political scene. Many were disputed by candidates ready to side with the government in order to obtain its support or, at least, its neutrality. By favoring one candidate, the authorities ran the risk of alienating his opponent, turning a merely opportunistic or otherwise indifferent notable into an open enemy of the regime.¹⁴ In the middle and later 1860s, as political activity intensified, reports reflect a growing awareness of the problem. In the southwest, where every parish seemed divided into rival "parties," such local divisions "with no political character" were affecting electoral results and attitudes toward the government. In the center of the empire, the attorney-general of Riom stressed "the considerable role played by personal and parish pump rivalries, particular interests and feelings," in the electoral struggles but added that such "shabby motives" afforded the opposition an opportunity to "spread political ideas among the people that they had hitherto ignored." And at Die, where the prepolitical coteries had adapted national issues to suit their own ends, the opposition, which was initially apolitical and local, soon became political and republican.¹⁵

Well into the Third Republic, however, rural elections were marked by the comparatively slight importance of political opinions. Indeed, the continuing importance of personal relationships may help explain the durability of many local potentates—scions of families "whose sole interest [had] always been the public good"—who remained ready to serve "whatever regime availed itself of their services." Not that the regimes had that much choice. In the Haute-Loire, from the First Empire to the opening decades of the Third Republic, eighty elections turned around thirty names. In other words, for well over three-quarters of a century, except during the years 1848–50, thirty families provided the high political cadres of the region.¹⁶ As Proudhon foresaw, universal suffrage was counterrevolutionary,

¹³ See AN, BB 18 1581 *passim*. Among many examples of the current discrimination between the "intelligent" few and the rest, see Préfet, Hérault, April 4 and July 31, 1866, and June 30, 1868, AN, Fic III 9.

¹⁴ On the elections at Tain (Drôme), see Grenoble, July 6, 1864, AN, BB 1717; and, on those at Tarascon (Ariège), see Toulouse, December 10, 1866, AN, BB 18 1692. Vincent Wright and Bernard LeClère have discussed the prominent part played by what a contemporary described as "rivalités locales . . . divisions de clocher . . . rivalités de position, des inimitiés de famille, des antagonismes d'influence et des haines de personnes"; LeClère and Wright, *Les Préfets du Second Empire* (Paris, 1973), 133. But, as one magistrate wrote in 1870, "If some day a true public spirit were to take shape, it will first be born at the municipal level"; Angers, July 4, 1870, AN, BB 30 390.

¹⁵ Montpellier, January 10, 1869, AN, BB 30 389; Riom, July 9, 1869, *ibid.*; and Grenoble, July 21, 1865, AN, BB 18 1717. To the same point, also see Limoges, August 12, 1865, AN, BB 18 1717; Montpellier, January 31, 1868, *ibid.*, 1755; and Préfet, Hérault, November 11, 1868, AN, Fic III 9.

¹⁶ Jean Goasguen, "Les Élections de 1871 et les manifestations de l'opinion publique en Gironde," *Revue historique de Bordeaux et du département de la Gironde*, new ser., 11 (1962): 11–12, 18, 19; and Jean Merley, *La Haute-Loire de la fin de l'Ancien Régime aux débuts de la Troisième République (1776–1886)* [hereafter, *La Haute-Loire (1776–1886)*], 1 (Le Puy, 1974): 480. On the crucial importance of personalities, also see the evidence of Baron de Bourgoing, AN, C 2874; L.-H. Roblin, "L'Administration d'une commune rurale," *Cahiers nivernais* (February 1909), 8; Michel Denis, *L'Église et la République en Mayenne, 1896–1906* (Paris, 1968), 74,

at least initially. But, although feudal-style patronage (noble or bourgeois) was widespread, its effectiveness was limited. Large landowners in small villages set the political coloring of the community. That coloring changed with time and varied from place to place, but for peasants to accept the landlord's politics as long as employment and social security depended on anything as "unimportant" as a vote made good sense.

Mayors, justices of the peace, constables, or highway inspectors were naturally—and rightly, too—regarded as fountainheads of troubles or favors. In 1865, an official writing from Brittany noted how eagerly rural electors went to vote—in sharp contrast to the numerous abstainers in more politically lively urban centers. Such civic punctiliousness, thought one magistrate, stemmed from "the influence the mayor can exercise in their private affairs." The peasant did not necessarily know the man he voted for; indeed, most often he did not. It sufficed that the mayor knew him. Reporting home from the Vosges in 1877, Jules Ferry rejoiced to find the peasants "cured of the sickness of fear." He was talking about a particular kind of peasant (*demi-bourgeois, indépendants par situation*); and, while the existence—and evolution—of their kind should not be ignored, many more retained "a sort of superstitious fear toward the owners of the land."¹⁷

Fear, respect, sympathy, dependency. For most of the 1860s and 1870s, men like the Baron de Veauce, elected deputy of the Allier in 1863 (as he had been in 1852 and 1857) by 17,930 votes out of the 19,061 votes cast, remained typical: "Born at Veauce, landowner at Veauce, mayor of Veauce, and quite simply Veauce. A little feudal baron. . . ."¹⁸ There were many like him elsewhere. As late as 1898, the Marquis de Lauriston, mayor of the small community in Loir-et-Cher, which even today has almost no roads, declared with confidence, "At Villefrancoeur there are no politics." What he clearly meant was, No one questions my word. The evidence suggests that, even in the twentieth century, many electors continued to vote for the

141; and Guy Thuillier, "Les Historiens locaux en Nivernais de 1860 à 1900," *Actes du 103^e Congrès national des Sociétés Savantes, Nancy-Metz 1978: Section d'histoire moderne et contemporaine*, 2 (Paris, 1979): 353–54.

¹⁷ Brittany, August 14, 1865, AN, BB 1717; and Ferry to his wife, June 1877, and to Jules Simon, November 1874, in [Ferry] *Lettres de Jules Ferry* (Paris, 1914), 226, 201. Also see July 6, 1861, AN, BB 30 370; and Eugène Ténot, *Le Suffrage universel et les paysans* (Paris, 1865), 26. For fear, see Préfet, Mayenne, to Ministre de l'Intérieur, May 31, 1898, as quoted in Denis, *L'Église et la République en Mayenne*, 68; and Françoise Raison-Jourde, *La Colonie auvergnate de Paris au XIX^e siècle* (Paris, 1976), 348. For confirmation by a connoisseur of rural politics of the regard in which local officials were held, see Pierre Joigneaux, *Nouvelles lettres aux paysans* (Paris, 1871), vi, 26–27. Also see Inquiry Commission, Gien, 1889, AN, C 5468. The C series is devoted to the minutes of and evidence gathered by Parliamentary Commissions of Inquiry into electoral improprieties or fraud and will be cited by the district of inquiry. In a letter of August 4, 1852, Gustave de Beaumont explained to Tocqueville the possible vexations he risked if he did not sit on his municipal council; Tocqueville, *Correspondance*, pt. 3: 66.

¹⁸ L. de LaCombe, *Profilis parlementaires* (Paris, 1866), 16. Also see the sketch of Brutus Cazelles, elected since 1852 at Lodève (Hérault), most recently by 28,495 votes out of 28,830 votes cast, and of Alexandre de Bosredon, deputy of Sarlat (Dordogne), "whose good people had confidently elected the man presented to it" by the government, although Sarlat probably "would have elected his competitor with the same confidence"; *ibid.* (2d edn., Paris, 1869), 96, 65. Government support—or its lack—could also influence results in the other direction: François Bel, a lawyer at Montmélian (Savoie), for example, was elected deputy of Chambéry South in 1876 and 1877, after the government had deposed him from his position as mayor; a general councilor of his district since 1860, "his position was very strong," according to Jacques Lovie, "a good example of those men of the second rank—prestigious, experienced, effective—who did more than the great politicians to implant the Third Republic"; Lovie, *La Savoie dans la vie française de 1860 à 1875* (Paris, 1963), 574.

man they knew, or as the man they knew told them to vote.¹⁹ If, by then, some voters picked up ideas that they had ignored in earlier days, they persisted in voting for something represented by someone: a notable and what he represented.

What he represented was often the *pays* and its “immemorial” orientations. The commonplace that more votes are cast *against* than *for* appears to be borne out by the long political history of local contentions: the antipathy between neighboring parishes; the hostility of rural areas toward the country town; the antagonism between village or *bourg* and the isolated hamlets and farms around them; or the dissensions of clans and factions based on family connections, service relations, or chance hostilities.²⁰ Such local allegiance and local loggerheads could not but affect behavior in the subsidiary realm of national politics. In Saône-et-Loire, around 1900, village rivalries drew “even stronger dividing lines than political rivalries, to the point that they often” muddled national politics. All over France many small towns (and their hinterlands) have been rivals “from time immemorial.” To repudiate a candidate merely because he was supported by another town, whatever his political label, was common. In Savoy, the little towns of Bonneville and Cluses, about fifteen miles apart, have disputed the leadership of the Faucigny area ever since the fourteenth century. Both towns voted Left, unless one town could vote against a candidate identified with the other. When that occurred, as it did in a famous election of 1897, the rival town voted enthusiastically the other way, unto its farthest parish.²¹

The politics of these endogamous societies appear little different in spirit from their family alliances. As proverbs warned against choosing a mate away from home, so electors were reminded that they could only judge a candidate’s worth if they knew him personally. He should be born in the area, live in it, “be bound to it by family bonds and everyday relations.” This particular statement dates back to the days of the *monarchie censitaire*, which often honored such requirements in the breach. Stendhal’s *Lucien Leuwen* testifies to the occasional success even then of what more recent days describe as *candidats parachutés*. In Isère, as the Empire drew to a close, Riondel, the mayor of St.-Marcellin, triumphed over forcible government opposition because the local patriotism of the cantons of Lower Isère gave him “an impregnable position,” according to the subprefect, “leaving all political considerations aside.” Yet in the “more advanced” cantons of Rives and Beaurepaire, which

¹⁹ Auguste Lacoïn de Villemorin, *Enquête sur la crise agricole* (Blois, 1898), 190; and Jean Lassaigue, *Figures parlementaires* (Paris, 1950), 27. For the mayor of an Ardèche village in 1908, amid his people “avid to guess his desires and conform to them,” see J. d’Indy, “Les Débuts du ski en Vivarais,” *Revue du Vivarais* (1953), 24; and, for village politics in the Briançonnais, see Emilie Carles, *Une Soupe aux herbes sauvages* (Paris, 1977), 211–14.

²⁰ For antipathy, see Maire, Colombiès, to Préfet, Aveyron, July 11, 1875, as quoted in A. Garric, *Histoire de Boussac* (Rodez, 1973), 83. For hostility, see “Rapport de l’élection d’Edmond Blanc,” 24, AN, C 5574; and Corbin, *Archaisme et modernité en Limousin*, 2: 1000–01. For antagonism, see Joseph Pinard, ed., “Les Mémoires d’un rural déraciné: Journal de J.-F.-S. Pinard,” *Mémoires de la Société d’Émulation du Doubs, Besançon*, 12 (1970): 8; and, for dissensions, see Grenoble, August 16, 1864, AN, BB 1717; Montpellier, January 30, 1866, *ibid.*; and Montpellier, January 1864, AN, BB 18 1685. Such venerable animus waned slowly; see Antonin Lavergne, *Monsieur le Maire* (Paris, 1905 [written in 1893–94]).

²¹ For Saône-et-Loire, see Ozouf, *La Classe ininterrompue*, 43. For references to Savoy, see Paul Guichonnet, “La Géographie et le tempérament politique dans les montagnes de la Haute-Savoie,” *Revue de géographie alpine*, 33 (1943): 39–86; Simone Hugonnier, “Tempéraments politiques et géographie électorale: Maurienne et Tarentaise,” *ibid.*, 42 (1954): esp. 45; and Lovie, *La Savoie dans la vie française*, 528–30.

might have been expected to support him even more strongly on ideological grounds but where he was less well known, the official candidate regularly gained more votes than he. In Savoy, the valley of Tarentaise and the hills of Maurienne have long opposed each other whenever possible. In the election of 1872, the candidate of the Left was a Dr. Jacquemoud of the valley town of Moutiers, in Tarentaise. His opponent was an ironmaster from the hill town of Randens in Maurienne. Most of the left-wing parishes in Maurienne voted Right, against the "man of the valley"; and a number of right-wing Tarentaise parishes voted for the Republican candidate, Dr. Jacquemoud, because he was their local man.²² Just two examples among many; but this fixation on an *enfant du pays* was not unreasonable since the nearer home the candidate, the more responsive to home needs he could be. Throughout the Second Empire and even into the Third Republic, no matter the political coloring of a commune or canton, the candidate who lived there or whose family lived there could garner 60, 70, or even 80 or 90 percent of the vote.

WHAT CHANGED, WHEN IT DID CHANGE, was the nature of the service a local patron rendered or the popular perception thereof. In a disputed election of 1876, in Haute-Garonne, the tone is still similar to that of mid-century. The supporters of Comte Adhémar d'Ayguesvives denied allegations of fraud; their candidate got his votes because he was very honorably known in the canton of Castanet, "because his family had owned properties there and still enjoyed great consideration." At a less exalted level, the mayor of Rebigue, accused of using the influence of his official position on behalf of Ayguesvives, replied, "If I enjoy any influence in the canton of Castanet, it is solely due to my family position and to my landholdings, not to the modest and obscure functions with which I have been entrusted." He also explained that his political opponents, who had testified against him, were his personal foes as a result of petty conflicts: one because the mayor had not saved his son from military service, another because he would not give him a job, the third because he had not leased him a piece of land.²³ True or false, this reflects the grass-roots realities of political partisanship. As the prefect of Haute-Loire had written in 1854, "There is no rivalry, no neighborhood squabble, that does not degenerate into open quarrels and suits, no quarrel not envenomed into hatred to be transmitted from one generation to the next." Such statements provide more than a hint of the grounds on which people chose sides. A family, a village, a social or professional group might have been Red or White because it had acquired national property in the 1790s, had taken communion from a Constitutional priest,

²² *Réflexions adressées à Messieurs les électeurs de Libourne* (Bordeaux, 1828); Pierre Barral, "Les Forces politiques sous le Second Empire dans le département de l'Isère," *Actes du Soixante-Dix-Septième Congrès des Sociétés Savantes, Grenoble 1952: Section d'histoire moderne et contemporaine* (Paris, 1952), 172; and Hugonnier, "Tempéraments politiques," 49. "The electors want to know and see their representatives in person," wrote one attorney-general; Besançon, June 27, 1861, AN, BB 1632. A report of the prefect in Pyrénées-Orientales, dated July 31, 1867, confirms this; the report is quoted in Horace Chauvet, *Histoire du parti républicain dans les Pyrénées-Orientales* (Perpignan, 1909), 188–89. Also see *Revue des candidats au collège électoral de Nevers* (Nevers, 1828), 7.

²³ Haute-Garonne, AN, C 3158. For similar arguments concerning the elections at Loudun five years later, see Vienne, 1881, AN, C 3324.

or had benefited from or suffered in the Revolution, and these attachments became solid, hereditary, intertwined with the material interest related to an economy of clientele and patronage.²⁴

This development was more readily apparent in the towns than in the villages and the countryside, where social cohesion remained stronger, alternative patrons (or opportunities of self-affirmation) more rare. The Revolution of 1848 and its aftermath began to change the bonds of both towns and the countryside, sometimes radically. Old motives and old factions subsisted, but their protective coloring took on new hues. Jean Dagnan, the historian of mid-century Gers, claimed that "universal suffrage awakened the countryside to political life." He showed villages dividing into Reds and Whites, "deux camps qui voisinent haineusement." But the political divisions merely fed on prior hatreds: "their discord is alimented by local intrigues and coteries, by family divisions." As some schoolteachers no doubt found, it became increasingly difficult to avoid choosing sides. A novel of 1860 makes the point clear:

Two parties, the White and the Red, struggle endlessly in the villages of the Midi. Willy-nilly, whatever a newcomer's indifference, he is enrolled in one of the two camps. . . . A simple quarrel with a member of his party is enough to cause a peasant to desert his camp. Politics are always the pretext of the disagreement, whose true cause really lies in some measly material issue.²⁵

So, by the 1860s, at least in the circumstances described, villagers had a choice: they could choose between parties (that is, between clans), and politics offered one opportunity to do so. Another novel (this one written by a village schoolteacher in the 1890s) describes how that might work. In the Languedoc village of Antonin Lavergne, the absence of a Republican leader had left the field to the Conservatives (note the modern political terminology!):

At last M. Guibal entered the field. Even though his family held Conservative opinions, he . . . openly declared himself the opponent of M. Lauret and his ideas. For many years, a veiled enmity divided the Guibals and the Laurets, who, even in the days of the First Empire, jockeyed for power and supremacy in Saint-Michel. Hence much friction and bad blood between the two families. Since then, the *bourg* divided into two camps: but political questions were excluded. After the fall of the Second Empire, the father of M. Guibal constantly opposed old M. Lauret in this way, while courting the Reds. [At his death, the son changed tactics.] Resolutely, he declared himself Republican; he was followed by his friends and became the party leader. Yet politics continued for a long time

²⁴ Préfet, Haute-Loire, 1854, AN, Fic III, as quoted in Merley, *La Haute-Loire (1776–1886)*, 445. For such continuities from the Revolution through 1851 and 1877 and even to the end of the century, see Gwynne Lewis, *The Second Vendée: The Continuity of Counter-Revolution in the Department of the Gard, 1789–1815* (Oxford, 1978), 230–31; and P. M. Jones, "Political Commitment and Rural Society in the Southern Massif-Central," *European Studies Review*, 10 (1980): 337–56.

²⁵ J. Dagnan, *Le Gers sous la Seconde République: La Réaction conservatrice (février 1848–2 décembre 1851)* (Auch, 1928), 170–71; and Mme. Louis [Juliette Bouscayet] Figuiet, "Le Franciman," in *Nouvelles languedociennes* (Paris, 1860), 119. Robert Smith has described village disputes as "waged with the peculiar vehemence of which only people who have known each other for a long time are capable"; *Kurusu: The Price of Progress*, 237. And he quoted Ronald P. Dore: "Competition within a group which is in theory harmoniously united tends to become fiercer and more emotionally involved than in one where competition is accepted as normal. As such it leaves scars after the event in the resentful humiliation of the defeated." Dore, *Land Reform in Japan* (London, 1959), 343.

extraneous to these local competitions, only formally referred to. . . . One was for M. Lauret or for M. Guibal, rather than Conservative or Republican.²⁶

In this light, village politics of the Third Republic look more like those of the Italian city-states of Romeo and Juliet than like the France of Jules Ferry. The voter's clan, the voter's leader, the voter's vote were designated by memories and decisions that remained obstinately local, whatever the superficial homage paid to the new national idioms. During the election of 1889, when the issue was the eminently modern phenomenon of General Boulanger, a local candidate in the Gironde was denounced by the Conservatives and supported by the Republicans because he was the grandson of a *sans-culotte terroriste*, who, even worse, had made money by buying "nationalized" property (*biens nationaux*). Further north, in Brittany, "One is Red or White, but one is necessarily one or the other. . . . Being born in a given family, in a given part of the parish, one inherits a political ideology."²⁷

Survivals of this sort were found at their most explicit in Corsica, where French officials and travelers never ceased to marvel at local anachronisms. One voyager noted in 1914 that there were "no political opinions in Corsica." He did not mean that the Corsicans were uninterested in politics per se but that they evinced no interest in French politics, which they did not consider their own. Their politics were very active indeed and affected local life in every possible way, but they were local, conducted in Montague-Capulet fashion and based on a system of patronage and clientele. Clients paid uneconomically low rents on the lands they held, lodged with the patron when they came to town, often asked for small loans without repaying them, sold the patron products he did not really need, and appealed to his influence when in trouble. In return, they voted for him faithfully and, if necessary, supported him with their guns. "If you live outside Corsica, you can follow a general policy. On the spot, these generalizations do not exist."²⁸ Note the exchange of services, the inescapable circumstances, and the irrelevance of alien generalities that have nothing to do with the local situations.

Corsica, in its stubborn anachronism, seems to have little to teach in the context of French mainland politics. But it was a museum of the values and processes of an only slightly earlier age. And similar Montague-Capulet situations on the mainland abounded—whether at Plozévet, where "M. Le Bail is Republican, hence we are Republicans because my father's family has belonged to the Le Bail clan for two generations"; or at La Réole, about forty miles upriver from Bordeaux, where from the 1880s to the Second World War local politics were dominated by the clan conflicts of *Larozistes* and *Chaignistes* who struggled for supremacy, directly or *par candidat interposé*, for three generations and thirteen elections.²⁹ As in Corsica,

²⁶ Lavergne, *Monsieur le Maire*, 318.

²⁷ R. Arambourou, *L'Arrondissement de La Réole au temps de la III^e République* (Bordeaux, 1962), 152; and André Burguière, *Bretons de Plozévet* (Paris, 1975), 203. As late as 1945, a Pyrenean elector told a candidate, "I am MRP, my poor father was for it already, and so was my poor grandfather." The MRP had just been formed, but the speaker meant it was the "good party" of his family. Quoted in Jacques Fauvet and Henri Mendras, *Les Paysans et la politique* (Paris, 1958), 502.

²⁸ Albert Quantin, *La Corse* (Paris, 1914), 263–74. Quantin's information was largely drawn from Paul Bourde's *En Corse* (Paris, 1887), 10–16, 39–46.

²⁹ Pierre-Jakez Hélias, *Le Cheval d'orgueil: Mémoires d'un Breton du pays bigouden* (Paris, 1975), 191; and Arambourou, *L'Arrondissement de La Réole*, 170–74.

whoever wanted a road surfaced, a school built, a subsidy granted, a contract awarded, a loan made, or a job offered had better vote right—and lucky.

As I read the evidence, which is admittedly patchy, the ideological and extra-local aspects of national politics remained the preserve of traditional leaders—great or petty notables—who used them for traditional ends. Only gradually did the country people themselves adopt new terminology, let alone new ideology, or perceive its relevance to their concerns. Even then, the extent to which local categories and local conflicts were simply rendered into a political language is difficult to determine; national alignments were often merely used to validate and support local interests and local divisions, in which political reality continued to lie.

ONE SIGNIFICANT EXCEPTION TO THIS APPARENT RULE concerns the Church: national (and international) ecclesiastical conflicts were quite swiftly and meaningfully translated into local terms. No historian of nineteenth-century France has ignored the importance of the clerical-anticlerical struggle, but only in the last decade have scholars like Maurice Agulhon and Ted Margadant stressed the major role this struggle played in the nationalization of local conflicts and the political edification of populations not accessible to ideological argument on other terms. But, although recent monographs (rightly) stress the conflict's historical role in the formation of the Left, some attention can usefully be directed to its contribution, quite simply, to the generalization of faction where occasions for it had otherwise been rare.

The sociopolitical importance of religious conflicts lies not so much in the divisiveness that set neighbor against neighbor but in the factionalism that set lord against lord and priest against priest, dividing authorities that were considered a single unit, that spoke with one voice, and that were in different circumstances natural to heed. The acquisition of national property during the Revolution and the retribution visited upon the buyers of this, mostly ecclesiastical, real estate after 1815 drove communities and families to the Left or into clericalism (generally of the Right) to cover their tracks. However compromised in the terror and counterterror of the revolutionary era or the White reaction of the Bourbon Restoration, these families, hitherto politically uncommitted, were forced into new political commitments for purely practical, not ideological, reasons. When not willing or able to melt into conformity, descendants of the farmer enriched by an opportunistic purchase or of the squire who had done well under the Convention or Directory while (or because) his fellows suffered provided a potentially new leadership in scores of rural communities. As the century progressed, tension also grew, here and there, between priests of vulgar extraction, limited in education and manners, and the *châtelains* ("squires" or "petty nobles") or other notables, who treated them as inferior dependents. Grudge, resentment, or vexation led some *curés* to set up as rivals of the *châtelain* or the mayor or to join some rival faction. More often, a local notable, an *acquéreur de biens nationaux* or descendant of one, headed an anticlerical or antinoble party.

As regimes kept changing, differences between the political sympathies of priests and notables became increasingly likely to occur and, worse, increasingly evident,

especially when official ecclesiastical policy went counter to the established sympathies of political patrons. Conflicts of authority had been rare at the grass roots. Under the July Monarchy, however, differences surfaced, with the strong clerical suspicions of Adolphe Thiers and, especially, of the "Protestant" François Guizot, when a multitude of parish priests got into hot (or tepid) water for refusing to sing the *Domine salvum fac regem*. Conflicts grew more frequent in 1848 and 1849, when universal suffrage, which propelled the humble masses into prominence, also invested their mentors with a new influence. Priests, far more than lowly schoolteachers, were the obvious beneficiaries of this new potential for leadership. Quite a few took up political positions that set a patron's teeth on edge, raised a portion of the community against them, and opened religiopolitical feuds that sometimes lasted for generations. Friction between local notables and the priest—with their respective factions that often arose from causes unrelated to religion or politics (school, the upkeep and decoration of church or presbytery, the parish budget, questions of precedence)—could also turn a parish into a political hotbed.³⁰

Such conflicts became more frequent still under the Second Empire. Cutting across custom and material interests both, the Roman Question determined the political attitudes of the clergy, whether turning new Bonapartists against their traditional legitimist protectors or sparking priestly hostility to a government unaccustomed to opposition from such a quarter. This led to open clashes between *cure* and *château* or between *cure* and *mairie*—sometimes, to clerical opposition to both at different times. Such built-in conflicts offered village priests opportunities for self-assertion for which many (especially of the younger generations) seemed to long. And noble patrons found it difficult to admit that a local priest's views should diverge from their own, whether he (before 1859 and in 1863 and 1865) supported an Empire they scorned or he (after 1871) opposed the regime they supported. In the Pas-de-Calais, for example, the nobles of St.-Pol were legitimist, but an "influential fraction" of the clergy were "clearly favorable" to Napoleon III.³¹ These conflicts also offered unexpected opportunities to mayors and other local officials at loggerheads with the parish priest to settle old scores in the guise of political action.

For the half-century after 1860, during which the Roman Question determined

³⁰ Préfet, Aveyron, to Ministre de l'Intérieur, June 27, 1834, AN, Fic III Aveyron, 5; Élie Reynier, *La Seconde République dans l'Ardèche* (Privas, 1948), 18; and Marius Faugeras, *Le Diocèse de Nantes sous la monarchie censitaire (1813–1822–1849): La Reconstruction catholique dans l'Ouest après la Révolution*, 1 (Fontenay-le-Comte, 1964): 232–39; and Gérard Cholvy, *Religion et société au XIX^e siècle: Le Diocèse de Montpellier*, 2 vols. (Lille, 1973), 1: 662, 701–07, and 2: 938. For a multitude of cases of conflict between priest and mayor, and of priests leading one of two village parties, see M. Dargaud, "Le Clergé bourbonnais en 1848," in Georges Rougeron et al., eds., *La Révolution de 1848 à Moulins et dans le département de l'Allier* (Moulins, 195), esp. 240–62. At Neufchâtel (Sarthe) in December 1851, the priest, leading a group of two hundred lumbermen of the Perseigne forest, marched on Mamers, and the marchers had to be dispersed by gendarmes; Paul Delaunay, *La Société sarthoise sous le Second Empire* (Laval, 1942), 41. For friction between local clergy and imperial authorities in Sarthe, see *ibid.*, 44.

³¹ Yves-Marie Hilaire, *Une Chrétienté au XIX^e siècle? La Vie religieuse des populations du diocèse d'Arras*, 1 (Lille, 1977): 313; and Cholvy, *Religion et société au XIX^e siècle*, 2: 560. For similar tensions in the Catholic West, see Michel Denis, *Les Royalistes de la Mayenne et le monde moderne (XIX^e–XX^e siècles)* (Paris, 1977), esp. chap. 9. And, concerning instances of clerical self-assertion in Mercourt, Haute-Saône, see Besançon, February 26, 1863, AN, BB 18 1666; in Chapelle-Blanche, Côtes-du-Nord, see Rennes, July 6, 1863, *ibid.*, 1675; in Hérault, see Procureur-Général, Montpellier, 1852, 1861, 1864, AN, F 19 5828; and in Somme, see Amiens, January 12, 1869, AN, BB 30 389. Cholvy has cited prefectorial complaints about the oppositional role of the clergy as early as 1857; *Religion et société au XIX^e siècle*, 2: 938–39.

the politics of the Church, the local stance of churchmen was more freely determined by new kinds of rivalry and friction, and their national politics were guided by the distant interests of papal policy. The priest as a free, or, at least, an unpredictable agent in politics was a newfangled phenomenon, and a troubling one. It was difficult to ignore priests like the one at Drouges (Ille-et-Vilaine), who told his congregation, "You must not vote for the government's candidate, you must not listen to the *gendarme*, or to the *cantonner*, or to any [public] agent."³² Here was a category of popular propagandist no regime, and no grass-roots public, could disregard—and one that, I suspect, did more to factionalize the country and to spread awareness of extra-local conflicts by tying them to local doings than the *montagnards* of 1849 had done.

After 1870, and especially after the fall of Rome, many clerics were more hostile to the Bonapartism they blamed for the "unpardonable" event than to the new Republic, and Bonapartists returned the antipathy in kind. This is no place for a survey of clerical and anticlerical attitudes. But it is worth remembering that priests' conflicts were not only with anticlericals. Priests could also be found opposing the conservative municipal council at Listrac (Gironde), quarreling with the White mayor of Cazédarnes (Hérault), feuding with the *châtelain*-mayor of St.-Jean-de-Cole (Dordogne), or even coming to blows with the local notary and president of the parish council at Boisse (Dordogne).³³ Predictably, the heightened tensions between different categories of notables were not only sensed by local populations but also reflected in their attitudes. These tensions, of course, grew worse with the formation of the Republic. But the real clash between Republicans and the Church came in 1877, when the Holy See appealed for aid from the French episcopate and from France in its conflict with the Italian kingdom. The Republican refusal to be dragged into a foreign quarrel (or the exploitation of the useful issue this offered) led to the crisis of May 16 and to elections fought with Gambetta's *new* slogan: "le cléricisme, voilà l'ennemi." We know all about that but less about the measures with which the Church sought to counteract its problems.

One result of the clergy's more consistently embattled stance toward the end of the nineteenth century was its increasingly deliberate entry into politics. "The mission of the clergy," wrote the bishop of Périgueux in 1904, "is to introduce Catholics into politics en masse." His statement may have been unusually explicit, but it came very late in the process of peasant politicization. The widespread campaign to persuade believers that voting in legislative or municipal elections was a religious duty was surely more significant than its more picturesque and, often, more familiar aspects: public damnation of political opponents, denunciation of pacts with the devil, sexual deprivation of politically peccant husbands, and so forth.³⁴ But these, too, contributed to the politicization of simple people—either by

³² Rennes, February 21, 1865, AN, BB 18 1692. Also see Préfet, Hérault (Montpellier), 1853, AN, F 19 5828.

³³ Goasguen, "Les Élections de 1871," 172; Préfet, Hérault (Montpellier), 1883, AN, F 19 5828; Pierre Pommarède, *La Séparation de l'église et de l'état en Périgord* (Périgueux, 1976), 141, 165; and Hilaire, *Une Chrétienté au XIX^e siècle*, 313–14, 339.

³⁴ François Delamaire, *Les Catholiques et les élections de 1906* (Périgueux, 1904), 20–21. Also see Pommarède, *La Séparation de l'église et de l'état*, 25. For the activities of a combative prelate like the bishop of Montpellier, Mgr. de Cabrières, long before that, see, among others, Cholvy, *Religion et société au XIX^e siècle*,

using idioms that were especially accessible and likely to impress or by the irritation and hostility they caused.

Another aspect of the same campaign may be viewed as more defensive in character. As early as the 1870s, the growth of anticlericalism and especially of religious indifference encouraged social Catholicism even among the least socially minded, with campaigns against Sunday work, so men could attend Mass, and for free seating in churches, so attendance would not be discouraged. More important still, charity wisely distributed was used to reaffirm poor people's confidence in their betters and to advance what a Béziers medical man described as "l'oeuvre chrétienne des élections."³⁵ Thus, Christian charity was enlisted in conservative politics as Republican enlightenment was enlisted in anticlerical politics. The two clashed, which they were meant to do, as in the 1880s, when manuals of moral and civic instruction by authors like Jules Steeg and Paul Bert were placed on the Index of Prohibited Books.³⁶ Some priests refused to admit children using the books in school to First Communion, with predictable results on village tempers.³⁷ Didactic inculcation of prejudice and its divisive fallout marched on.

To this, the Catholic publicists contributed. The 1890s were marked by the appearance of regional editions of *La Croix*, which appealed to country people not only through their local news furnished by village priests and practical advice about hygiene and crops but also by their tone. *La Croix* was republican and patriotic, but it opposed capitalism, financiers, speculators, Jews, and foreigners. Combative advocates of the Catholic cause, the paper's writers were forceful participants in the religiopolitical debate and advanced interest in politics by making both religion and politics more lively and intriguing. Like their competitors of the *mauvaise presse*—*Dépêche de Toulouse*, *Progrès de Lyon*, *Petite Gironde*, *Petit Parisien*—such papers were introduced by long-term, free "trial" subscriptions, followed by subscriptions at reduced prices. Whatever their rural readership, and it continued to be low until after the First World War, around 1905 most village priests read and recommended *La Croix*. By then, however, the political struggles of the preceding years had

2: 1118–19. For succulent examples of clerical pressure from 1876 to 1903, see Alexandre Pilenco, *Les Mœurs du suffrage universel en France, 1848–1928* (Paris, 1930), 169, 197, 200; and, for a discussion of electoral pressures in the district of Mauléon (Basses-Pyrénées) in the 1880s, see *Journal Officiel*, December 22, 1889. *La Croix*, May 11, 1903, gives the text of a contract between the Devil and Achille Fould, the banker, ensuring the latter's election in the Hautes-Pyrénées.

³⁵ For fascinating detail on this score, see Cholvy, *Religion et société au XIX^e siècle*, 2: 1275–76.

³⁶ See, especially, March 19, 1883, AN, F 19 5828. Also see Cholvy, *Religion et société au XIX^e siècle*, 2: 1115. For the *affaire des manuels* at Plozévet, see Burguière, *Bretons de Plozévet*, 240, 284–86. Of course, the Third Republic went in a great deal for history written for politically didactic ends, like the politicized version of Albigenian history used as an instrument of anticlericalism. Napoléon Peyrat's three-volume *Histoire des Albigeois* appeared from 1870 to 1872. For Peyrat, a Protestant minister, Catharism was a forerunner of Protestantism and of the revolutionary ideal of freedom, with the Albigenian Crusade representing the bloody intolerance of the Roman Catholic Church; see his *Réformateurs de la France et de l'Italie au XII^e siècle* (Paris, 1860). This was the version of thirteenth-century history that the schools taught for several generations, helping to turn the Midi to radicalism and also reinforcing a resentment against the conquering North that surfaced in the troubles of 1907. Also see Thuillier, "Historiens locaux en Nivernais," 358: "Assurément, il existe une histoire locale de droite et une histoire locale de gauche."

³⁷ First Communion was a crucial rite of passage, and some employers long continued to require First Communion certificates before they accepted lads into their service. See, for example, Yves-Marie Hilaire, "Remarques sur la pratique religieuse dans le bassin houillier du Pas-de-Calais dans la deuxième moitié du XIX^e siècle," in Louis Trénard, ed., *Charbon et sciences humaines* (Paris, 1966), 270.

developed a taste for even stronger fare: *La Libre Parole*, *L'Autorité*.³⁸ Clerical politics opened the door to national politics.

ALL OF THIS HAS TAKEN ME FAR AHEAD OF MY TALE, but not away from it. Like the *démocs-socs* of the Second Republic, priests carried their ideologies and their peculiar politics into areas where these had been unknown. But the conversions of mid-century were never so lasting as those achieved two or three decades later, when continuity of endeavor found a material context to support its aim.

What the Second Republic left behind in limited circles, even in the countryside, were the open wounds of the repression that began in 1850 and culminated in the aftermath of December 2, 1851. The great Revolution and the Old Regime had left their imprint too: deeply etched memories and family alignments that did not easily diminish. During the Second Empire and much of the Third Republic, tithe and *corvée* retained their power to horrify and mobilize to electoral ends. So did resentments produced by 1851. In 1856, the subprefect of Béziers reported both unhealed, with the “personal, family and party hatreds” they had caused likely to last a long time in a region haunted by “the hatreds and discord of 1815.” Those hatreds were certainly still there in 1870. The Empire, which sought to dampen political tensions among the peasantry, had done little to assuage resentments among its erstwhile opponents in the middle and lower-middle classes. Teachers, postmen, tax collectors, and *cantonniers* denounced and revoked under political pretexts—as Whites before 1849 and Reds thereafter—had learned political lessons that they earlier had most probably ignored.³⁹ During the winter of 1858–59, after Orsini’s unsuccessful attempt to murder the emperor, some two thousand suspects were arrested, over four hundred of whom were transported to Algeria.⁴⁰ Such political hatreds as had subsided in the intervening decade were revived by the renewed brutalities and suffering of those months.

These actions do not seem, however, to have reverberated far among the peasantry. Few, like Jean Fontane of Anduze (Gard), continued to be “constrained to political inactivity but dreaming always of revenge” through the decades that

³⁸ J. Calvet, “Une Monographie religieuse d’un diocèse français: Diocèse de Cahors,” *Revue catholique des églises* (February 1905), 70; and G. Rouchy, “Monographie religieuse d’un diocèse français: Le Diocèse de St. Flour,” *ibid.* (November 1905), 539, 549–50. It is important to remember that, where the violent rural clashes of the Inventaires occurred in 1906, they reflected rather a clinging to collective traditions (and collective property) than the clash of “modern” political clerical-anticlerical forces; see Jean-Marie Mayeur, “Géographie de la résistance aux Inventaires,” *Annales: Économies, sociétés, civilisations*, 21 (1966): 1270–71; and Pommarède, *La Séparation de l’église et de l’état*.

³⁹ Sous-préfet, Béziers, January 1856, AN, Fic III, Hérault 9. In the elections of 1869 one candidate in the Isère averred that, if his opponent won, men would be yoked to the ploughs in lieu of horses, with the iron collars that his opponent’s grandfather had used for that purpose before the Revolution; *Moniteur universel*, December 12, 1869, 1596: The crowd cried out, “Nous ne voulons pas labourer.” Also see, for the elections of Haute-Garonne in 1873, Charles de Rémusat, *Mémoires de ma vie*, 5 (Paris, 1967): 481; and, for the Drôme in 1865, see Grenoble, August 10, 1865, AN, BB 18 1717; and, for 1870, Comte Charles de Lazerme, *Carlistes et légitimistes* (Perpignan, 1937), 63–64. In general, see Edward Mauve, *Le Bourbonnais sous la Seconde République* (Moulins, 1909), 109–21.

⁴⁰ Eugène Ténnot and Antonin Dubost, *Les Suspects en 1858* (Paris, 1869). A memo of the minister of the interior, dated March 29, 1864, raises the question of persons condemned after the Vendean risings of the early 1830s, released or granted amnesty since then but still continued under police surveillance in the 1860s; AN, BB 18 1960.

followed. Perhaps the political interests awakened in some villagers during the Second Republic did indeed lie dormant under the Second Empire, awaiting only the chance to spring back to life. Yet the evidence I have seen suggests less a prudent dissimulation than an absence of sentiments needing to be dissimulated.⁴¹ The Empire did not foreclose all opportunities of expression for the political classes, however fruitless these might actually have been in practice. But I have seen nothing to indicate that the silent masses kept silent because of repression rather than out of indifference.

This is the context in which we should look at a process of political evolution as varied as the multitude of parishes and clans making up that jigsaw—France. The Second Empire's contribution to the politics of notables was the official candidate, who was selected by the government's representatives and supported by the government's not insignificant resources and prestige: "a light that opinion needs to guide it amid party intrigues." Through the 1850s the light shone largely unimpeded—its effects, as Quentin-Bauchart explained, "straightforward and free of pitfalls."⁴² The peasants needed firm leadership. Opponents impertinent enough to enter the electoral lists were treated cavalierly: their posters were prohibited or torn down, the men distributing their voting bulletins harrassed and sometimes jailed, their complaints ignored. In the following decade, in small parishes at least, shouting one's support for the wrong candidate could earn the voter a beating followed by imprisonment. Officials of the *Ordre Moral* saw things no differently: in 1877, the subprefect of Mauriac (Cantal) instructed all mayors of his district that distributors of republican tracts were acting illegally and should be arrested and conveyed at once to Mauriac. As a prefect of those days insisted, "Il faut imposer le bien."⁴³

Given a peasantry absorbed by work and reasonably satisfied with its returns, the policy worked. Many villagers who stirred to defend the Republic in 1851 probably came to believe the government's argument that they were misled, since they were not personally affected by Napoleon's coup d'état. Social superiors, who might have enlightened them, had been eliminated or held their peace.⁴⁴ Then came the gradual relaxation of the 1860s. Although the liberal Empire was introduced only in 1867, villagers could after 1859 again hear their betters speak with several voices, offering alternative leadership and courses of action. Republican candidates appeared in the elections of 1863 and by 1865 and 1866 magistrates were reflecting on the "emancipation" of the electorate. Electoral results were still attributed (and

⁴¹ Jean Fontane, "Souvenirs d'un paysan," *La Révolution de 1848*, July–August 1909, pp. 165–70. Fontane had organized and briefly raised his friends at Massillargues and Atuech in December 1851. But see Paul Muller, "Autour du coup d'état dans le Haut-Rhin," *ibid.*, September–October 1901, p. 211: The *démoc-soc* tendencies, so strong in the Haut-Rhin from 1848 to 1850, "were no more than a historical recollection" by 1869.

⁴² Poitiers, April 6, 1863, AN, BB 18 1671; and Quentin-Bauchart, *Études et souvenirs sur la 2^e République et le Second Empire (Mémoires posthumes)*, pt. 2 (Paris, 1902): 8. For straightforward support of the waning practice, see Evariste Bavoux, *Les Candidatures officielles* (Paris, 1869).

⁴³ Sous-préfet, Mauriac, 1877, AN, BB 18 1567; and Préfet, Poitiers, July 16, 1866, AN, BB 18 1692. Also see Pilenco, *Les Mœurs du suffrage universel*, 58, 89, 112.

⁴⁴ See AN, BB 30 368 (the years 1855–63). For confirmation, see Ténot and Dubost, *Les Suspects en 1858*, 22, 23.

probably with justice) to “petty local passions” and, hence, often dismissed as “politically” insignificant. Yet it was recognized that “universal suffrage is no longer, as it was a few years ago, in the hands of the administration,” for voters had come “under the direction of other influences.” Public opinion, “even in the countryside,” was “awakening”—increasingly “determined to find its own direction.”⁴⁵

Patchily but portentously, growing prosperity was beginning to emancipate rural populations from traditional dependencies—and absorptions. Like the offspring of the successful Var farmers turned cork manufacturers in the 1840s, whom Maurice Agulhon has studied, the sons of thriving Languedoc wine-growers now sought to assert their independence from the local gentry. Hitherto “white,” they signaled their newfound aspirations (and their up-to-dateness) by flaunting Republican sympathies. Established forms of sociability provided them with a ready forum, and the burgeoning labor shortage provided them with a potential following of journeymen emancipated from the landowner’s sway. Where such conditions obtained, the villager with pretensions or a grudge now had somewhere to turn. In 1858, the attorney-general of Montpellier noted the trend, only just beginning, philosophically: “les jeunes veulent être de leur temps.”⁴⁶

Adjustment to this novelty did not come easily. The sensitive Charles de Rémusat, Thiers’s future minister of foreign affairs, had realized as early as 1863 that electoral success now called for unwonted effort, for a far greater investment of time and energy than before. He was ahead of his time; but other astute observers soon recognized the “urgent” need to work on the electorate, not intermittently but constantly, from well-established *local* bases. If constituencies were to be won, or held, they had to be nursed. As early as 1861, a magistrate noted that at Marcheux (Doubs) the governmental candidate, M. de Conegliano, was resented because, though a local landowner, “he rarely sees his electors.” The local electors had turned to a rival “who spends his life among them.” New expectations were taking shape, and wise politicians drew the consequences. Rémusat’s brother-in-law, Adrien de Lasteyrie, settled at Lagrange (Seine-et-Marne) where in 1863 he had run a close race against the official candidate. Auguste Casimir-Perier, elected to the General Council of the Aube where he owned considerable property, took a house in Troyes and spent all winter in the region in order to prepare for the 1869 elections.⁴⁷ Gone were the days when Godefroy Cavaignac, recognized and feted in a country inn, could refuse to shake hands with clodhoppers (*bousingots*) because, as he explained, he did not know them.⁴⁸ When in 1869 the Marquis de Grammont

⁴⁵ Pau, April 2, 1866, AN, BB 18 1717; Grenoble, August 10, 1865, AN, BB 18 1755; Pau, April 2, 1866, AN, BB 18 1692; and Lyon, August 8, 1867, AN, BB 18 1755. Margadant has stressed the youth of secret-society militants in 1851; *French Peasants in Revolt*, 183. Thus, they were still around and available in the later 1860s, when political activity resumed and opportunities for sustained action had much improved.

⁴⁶ Agulhon, *La République au village*; and Montpellier, July 7, 1858, AN, BB 30 380. For Languedoc, see Cholvy, *Religion et société au XIX^e siècle*, 2: 1033, 1039, 1053.

⁴⁷ Rémusat, *Mémoires de ma vie*, 159, 237; Rouen, August 11, 1865, AN, BB 18 1717; and Besançon, June 27, 1861, AN, BB 18 1632.

⁴⁸ Alexis de Tocqueville noted the story on June 25, 1856, as told to him by Cavaignac himself; *Correspondance*, pt. 3: 419.

⁴⁹ Besançon, June 26, 1869, AN, BB 1784; and L. de LaCombe, *Profilis parlementaires, 1863–1869* (rev. edn., Paris, 1869), 107. Others adopted similarly vulgar but more ingenious methods of garnering public favor. Thus, for example, the handsome Stephen Liégeard, elected deputy of Thionville (Moselle) in a by-

was elected in the arrondissement of Luxeuil (Haute-Saône) and his peasants flowed into the *bourgs* to celebrate, the attorney-general of Besançon reported ruefully that "the marquis himself intoxicated by his triumph, mixed with the popular rabble." Others of less exalted nobility, like Flocard de Mériepieu, mayor of Sarmérieu and deputy of the Isère since 1852, even went so far as to lead the local fire company.⁴⁹

A new step toward democratic politics was taken in 1868, when electoral meetings were authorized under fairly strict conditions. A mass of official reports speaks to the concern felt by police and magistrates about the appearance and extension of "ostensibly" private meetings and about the surveillance and harassment to which opposition figures were subjected.⁵⁰ As yet, however, only local notables were affected. Meetings by personal invitation scarcely touched the humbler electors—and certainly not outside the industrial centers.⁵¹ News of effective political campaigning, nevertheless, spread to fairs, taverns, and *veillées*—even into Haute-Savoie where tales of Jules Favre's oratory evoked the admiration of mountaineers who had never heard him. "We must be careful to vote for [him]," they said. "At least he's not dumb." Immediately effective or not, this feature of political contests became increasingly significant; politics as entertainment attracted a rural population woefully short of diversions and eager to break the monotony.⁵²

At any rate, the later 1860s showed evidence of a new—and recognizably political—sensitivity to issues of wider import. Given a little time, mused the attorney-general of Aix in November 1868, attitudes hitherto limited to urban centers would trickle into the countryside, and the opposition views of a politicized urban middle class "would progress rapidly among the rural population." The peasant was more reluctant to accept administrative suasions, reported the attorney-general of Dijon a few months later. It was not that the peasant had developed political views but that he wanted to assert himself; thus, he tended to prefer the opposition. His colleague in Besançon summed up the evolution:

Parliamentary ideas, once discredited, have regained [public] favor . . . ; the movement that began among the upper classes is affecting the masses: the practice of universal suffrage and the reading of newspapers have altered their frame of mind. They understand the power that elections give them and insist on using it as they please. We shall henceforth have to take this new tendency into account.

One magistrate in Toulouse even went so far as to consider the uses of a new war

election of 1867, used photographs: "Sa photo . . . largement distribuée par un barbier lorrain a beaucoup contribué à son élection. On l'a nommé sur carte." *Ibid.*, 156.

⁴⁹ See AN, BB 1795¹ and 1795², *passim*.

⁵⁰ Even there, the democratic leadership had its doubts about the workers whom it wooed. At one public meeting held at Béziers and addressed by Charles Floquet, a workman climbed on the stage and asked to make a statement. The chairman and Floquet prevented him from speaking: "Qu'est-ce qu'il veut?" the chief of police heard Floquet say to his friends on the platform. "Arrêtez-le, il va nous faire des bêtises." Montpellier, May 7, 1869, AN, BB 18 1795¹.

⁵² Quoted in Paul Guichonnet, "Jules Favre et la bataille politique: Les Élections de 1869 en Haute-Savoie." *Cahiers d'histoire*, no. 1 (1956): 90. Note that Favre desisted in favor of the "Liberal" Monarchist-Catholic candidate, who won. Also see the conclusion of George Baume's novel *M. le député* (1909), about the election in a small country town where, the campaign over, all moan, "What are we going to do now?"

in turning opinion away from "these sterile and irritating questions of internal politics."⁵³

By late 1869, in the midst of official reports that confirm the continued lack of interest in country people, nuances began to appear: "Our countryfolk have not departed from their usual indifference," advised the attorney-general of Limoges. "Generally illiterate and until now practically indifferent to the political impulse spread by the press, they are really only interested in promoting their own material well-being, selling their products, and improving the means of communication, whose constant development leads them to appreciate governmental activity." Here are new themes that gained in importance with the years, themes not unrelated to a novelty, already noted in this essay, which struck the attorney-general: "the importance the peasants are beginning to attach to the exercise of their electoral rights."⁵⁴

Economic expansion and affluence were propelling once-isolated regions into the national market, suggesting new needs and possibilities directly related to the national government and, hence, to electoral politics, which the rural areas had once ignored. Electoral politics catered not only to self-interest but also to dignity, suggesting novel possibilities for self-affirmation. The farmers in Bordeaux, for example, were "very flattered to find themselves the object of the visits and the consideration of candidates who came to ask for their votes. Some peasants, imagining themselves very enlightened about their rights and their [new] importance, took advantage of this . . . to thwart the local authorities." These were mere forest murmurs. As the attorney-general of Chambéry explained, the problem was not that of getting the "peasant agitated about political issues during an electoral campaign" but of finding someone with "reasons to do it. . . . In the end, the bourgeoisie alone has the enlightenment and the money needed to move the masses."⁵⁵ The bourgeoisie soon entered the lists en masse but began by finding, if we can believe *La République de l'Allier* of September 26, 1870, "rural populations unenlightened, scarcely literate, and obey[ing] in politics more instinct than reason."

SO WE MUST NOT EXAGGERATE isolated developments or the perceptions of timorous officials, excessively sensitive to anomalous displays of defiance. Republicans continued to insist on the peasants' political ignorance and the need to undertake their political education. More important, perhaps, and certainly more immediate, Republicanism remained an urban phenomenon, lacking militants at the grass roots. Indeed, had there been more Republican notables (or other enthusiasts), the Republicans would have felt less keenly the need to educate the peasant into political autonomy. And the plebiscite of May 1870 seemed (and seems) to bear out

⁵³ Aix, October 7, 1868, AN, BB 30 389; Dijon, January 13, 1869, *ibid.*; Besançon, January 12, 1869, *ibid.*; and Toulouse, October 6, 1868, *ibid.*

⁵⁴ Limoges, October 1, 1869, AN, BB 401². For the traditional theme of calm and tranquillity, see Angers, October 1869, *ibid.*; Besançon, October 1869, *ibid.*; and Ténor, *Suffrage universel et les paysans*, 23–24.

⁵⁵ Bordeaux, October 1869, AN, BB 401²; and Chambéry, October 1869, *ibid.* On the continuing, crucial role of the rural bourgeoisie, if this needs to be bolstered further, see André-Georges Maury, *Histoire de l'Auvergne* (Toulouse, 1974), 390; and Roger Pierre, *Les Origines du syndicalisme et du socialisme dans la Drôme* (Paris, 1974), 23.

those who saw the rural electorate as scarcely politicized.⁵⁶ The *candidature officielle* itself continued well into the 1870s. Yet it did not work as well as it once did. "Our good peasants," insisted a Bonapartist in 1874, "are easy to lead only when one leads them where they want to go."⁵⁷ That may always have been the case, but now it had to be reckoned with. Slowly—in some places very slowly—the traditional notables left the countryside;⁵⁸ meanwhile, a new figure entered the scene: "The politicians are invading France," declared a publicist in 1879. "The term is new, because the phenomenon is new."⁵⁹

More clearly than under the Empire, effective local patronage came to be seen as dependent on broader connections—to the Paris world of banking, press, and politics, which provided publicity for local grievances, support for municipal needs, or favors for private electors. The availability of such connections and the perception of their significance dominated the gradual shift from simple factional alliances to interest group politics.⁶⁰ Influencing this slow evolution was the Republican campaign to win over the indifferent or unfriendly masses, without whose support prospects were bleak. In the Nièvre, where the countryside was "under the tutelage of the great landowners and dependent upon them," a Republican politician, who was the son and grandson of Republican notables, insisted that in 1870 rural electors "had no personal opinions; they expected to be told." To modify a public opinion largely won over to the Empire and bring it over to Republican ideas, "a whole education of the electoral body" was needed. Very quickly, the Republicans set out to improve their position, especially in the rural regions, "by enlightening rural electors on their rights and duties in electoral matters." Result: "at no time was political life in the department so intense."⁶¹

⁵⁶ See Ténot, *Suffrage universel et les paysans*, 30, 27; Alfred Massé, *Les Partis politiques dans la Nièvre de 1871 à 1906* (Nevers, 1910), 11–12; Denis, *L'Église et la République en Mayenne*, 29; J. Cornillon, *Le Bourbonnais à la fin de l'Empire et sous le gouvernement de défense nationale* (Moulins, 1924), 28; Louis Girard et al., *Les Conseillers généraux en 1870* (Paris, 1967), 80, 144. Paul Delaunay claimed that Sarthe peasants, who voted heavily for the Empire, were lost between the significance of yes and no in the plebiscite; *Société sarthoise sous le Second Empire*, 76. *La Feuille du village* of May 21, 1870, describes them as *effarés et ahuris*. One might ponder the remarks of Eric Rouleau on Iran, in an article in *Le Monde*, December 12, 1979, p. 3: "In a country that has practically never lived under a democratic regime, the major part of the population has no sense of living under a dictatorship. . . . That is to say, the (sometimes serious) encroachments on their liberty of which liberal intellectuals are sometimes the victims do not especially move most of the Iranians except when [such encroachments] go directly against their interests or aspirations."

⁵⁷ Fernand Giraudeau, *Vingt ans de despotisme et quatre ans de liberté* (Paris, 1874), 64–66.

⁵⁸ As early as 1869, "nobles, once omnipotent in the Vendean countryside are no longer the only masters in their communes"; M. Fauchoux, "La Vendée," in Louis Girard, ed., *Les Élections de 1869* (Paris, 1960), 150. Beginning in the 1860s and accelerating thereafter, more and higher taxes, rising wages, a thinning labor force, and better investment opportunities elsewhere persuaded landowners—noble or bourgeois—first to let and then to sell their properties. A tenant farmer, observed Gabriel Boscary, who could cut costs and live cheaply himself would make a profit on a holding on which the master would ruin himself; Boscary, *Évolution agricole et condition des cultivateurs de l'Aveyron pendant le XIX^e siècle* (Montpellier, 1909), 86. Also see Comte de Comminges, *Souvenirs d'enfance et de régiment* (Paris, 1910), 158; and Cholvy, *Religion et société au XIX^e siècle*, 2: 1102.

⁵⁹ Georges Lachaud, *Voyage au pays des blagueurs* (Paris, 1879), 1. The term "politician" appears to have been re-introduced around 1865, to be used pejoratively about Americans in public life. Émile Littré did not mention it, but the *Supplément* of 1877 explains that the word, until then exclusively used with reference to the United States, was beginning to enter common usage for France; Littré, *Dictionnaire de la langue française* (Paris, 1876), and *Supplément* (Paris, 1877), 270.

⁶⁰ For examples, see Arambourou, *L'Arrondissement de La Réole*, 170–73; Jean Pataut, *Sociologie électorale de la Nièvre au XX^e siècle* (Paris, 1956), 123; Sous-préfet, Riom, July 29, 1893, Archives départementales [hereafter, AD], Puy-de-Dôme, M. 0162; and *Journal de la Nièvre*, May 8, 22, 1906.

⁶¹ Massé, *Les Partis politiques dans la Nièvre*, 12, 23. Alfred Massé, who became a senator of the Nièvre in 1879, was the grandson of a Republican notable and militant of the Second Republic and, hence, the scion of a typical

Political life had been equally intense in the period after the Revolution of 1848, but it had been restricted physically (fewer villages had been touched by it), culturally (illiteracy and a sense of irrelevance limited its impact), and practically (real alternatives were few). At that time Republicans and Bonapartists had sought to indoctrinate villagers and draw them into the political game. I have argued elsewhere that conditions were not ripe, that the context of political activity under the Second Republic was still archaic, and that modern ideas wilted in an alien world where they were only perceived, if they were perceived at all, in outmoded terms.⁶² Since that time, more roads, railways, and schools had been built, and opportunities for urban employment had begun to moderate the endemic under-employment of country people, improve their wages and autonomy (however relative), and lighten their dependence on landlord and neighbor.

Such generalizations only apply, where they apply, in the most general sense. Years, sometimes generations, passed before the peasant felt their effects and drew their political conclusions. But the Second Empire and the ten to fifteen years that followed laid down a material base for the evolution of modern politics. Thereafter, the efforts to mobilize the peasants had the advantage of continuity, and the voting public was better prepared to listen to, and to perceive the relevance of, ideas no longer entirely strange and unrelated to local and individual interests. Even so, the countryside did not spontaneously enter the wider political arena but needed to be dragged there. As the attorney-general of Chambéry had predicted, someone had to invest the money and the energy necessary to move the masses. After 1870, all urban political groups turned to that task: the Republicans in order to establish solid rural positions, the Conservatives in order to retain theirs. Political exposure creates political experience and under the Third Republic, especially in its first decades, such experience did a lot to advance the politicization of the countryside.

The most significant results were produced by the sharpest political crises—during elections that were fiercely contested and when contestants scrambled for support in quarters previously ignored. Comparing the election of 1877 to that of 1871 in the Nièvre, Massé concluded that the Conservatives “owed their relative success [in 1877] to the means they used to bring to the polls a large number of electors *who had not voted in the preceding elections*.”⁶³ But the most marked results were achieved in the elections of 1889, which seem to have mobilized parts of the countryside as never before. It is true that adverse economic conditions contributed to this mobilization. The agrarian depression of the 1880s had soured many on Republican patronage, previously accepted as more serviceable than that of Bonapartists or Legitimists. Hard times sharpened the farmer’s expectation of

local political dynasty. For examples of political didacticism, see the *Supplément au Républicain de l’Allier*, October 4, 1877, which shows in great detail the legislation and civic rights related to elections. Also see AN, C 3229. Republicans had also sought to enlighten the rural masses in 1848 and had founded papers “destined to make the political education of our countryside”; *Journal du Tarn*, March 11, 1848. The results had, however, been less than lasting; see M. Greslé-Bouignol, “La Révolution de 1848 dans le Tarn,” *Revue historique et littéraire du Languedoc* (1948): 19, 294.

⁶² Weber, “The Second Republic, Politics, and the Peasants,” *French Historical Studies*, 11 (1980): 521–50.

⁶³ Massé, *Les Partis politiques dans la Nièvre*, 34 (italics added). It would be interesting to know if Bonapartist efforts in the early 1870s went far beyond the towns. Their *Comité de l’appel au peuple* was designed to create, or re-create, a departmental organization and spread Bonapartist propaganda in the provinces. See [C. Savary] *Rapport de M. Savary sur l’élection de la Nièvre* (Paris, 1875), *passim*. The Bonapartists attempted, according to

material returns from his vote. But the political activity itself was surely as important as the ends to which it was deployed; and, again, as in 1877, an activist “Bonapartist” approach (now enriched by the infusion of American campaign practices) sparked unusual interest and participation.

In districts where they ran, Boulangist candidates and their propagandists went everywhere. The copious campaign chests of the Boulangists meant that villages never before touched by political debate (their political orientation had always been taken for granted) were now riven by rival arguments and new divisions. Thus, a Boulangist candidate in Corrèze, who decided to by-pass a particular village assumed to be adverse to his candidacy, reversed his decision on learning that he had a chance there and was gratified to find supporters. Because political orientations were confused by Republican and Legitimist alliances with Boulanger, people who had steadfastly and “naturally” voted republican, clerical, or conservative had to reconsider their allegiances. At the same time, Boulangist propaganda sought to relate local interests more closely to national issues. As a result, a hostile witness averred, the idea spread to “the humblest cottage . . . that governmental action could modify everyone’s way of life.” This was a revolutionary notion to communities where, for centuries, government had been regarded as a distant, hostile force. “For a very long time in this area,” a local official in the Pyrenees had reported in 1850, “country people have taught the children to consider the government, any government, as their natural and personal enemy.”⁶⁴ This fundamental attitude had to change before national politics could gain attention. Relating government to the fulfillment of the people’s needs made politics relevant and connected parliamentary elections with self-interest.

A more immediate result of Boulangist campaigns, however, was the appearance of rural syndicalism. In Cher, Nièvre, and Allier, the Boulangists focused on the peasantry, especially on lumbermen hurt by deteriorating economic conditions but hitherto indifferent to politics. Until then, Louis-Henri Roblin, one historian of their unions, has claimed, agricultural workers and lumbermen regarded political debate as “a question of men rather than of doctrines.” Lumbermen had never before thought that, for them, their work, and their pay, the form of government could have any importance. Newspapers were only read in the industrial centers or by the more well-to-do in the countryside. Public meetings did not attempt to address the rural workers. “With Boulangism everything change[d]”; Boulangist successes in central France in 1889, Roblin pointed out, were followed closely by the spread of peasant strikes and unions (1891) and the eventual electoral success of Socialists and Radical-Socialists (1892).⁶⁵

Two points are worth noting about this. The first, perhaps a minor reminiscence, is the similarity between the effects of Boulangism, “which left a prodigious echo in the popular consciousness,” and those of democratic socialist propaganda two score years before. Writing in the aftermath of the 1890s, Amédée Dunois, who had been

Savary, to get people involved in public activities (addresses, petitions, pilgrimages, and so forth), a list of whose participants they published, “which would commit [the participants] in the future”; *ibid.*, 9.

⁶⁴ 2d district Tulle (Corrèze), 1889, AN, C 5470; Pataut, *Sociologie électorale de la Nièvre*, 82; and Sous-préfet, Ceret, February 1850, AD, Pyrénées-Orientales, as quoted in Peter McPhee, “The Seed Time of the Republic” (Ph.D. dissertation, University of Melbourne, 1977), 461.

⁶⁵ Roblin, *Les Bûcherons du Cher et de la Nièvre: Leurs syndicats* (Paris, 1903), 96–98, 314.

involved in these events, insisted on the great hopes they had evoked: "All honestly imagined that the time of salvation was near and that the miseries, the oppression, the humiliations of all kinds . . . would end forever." The eschatological belief in a brave new world that could begin at once, so prominent in the peasant aspirations of 1851, was soon again evident in the appeals that swayed and mobilized unsophisticated country people, introducing—or re-introducing—they to *political* activity. The second point is that, while local political figures who had sided with General Boulanger lent the lumbermen a hand, their movement was a peasant movement with peasant leadership. Its very lack of order and of coordination (in 1891–93 lumbermen on the two shores of the Allier ignored each other's activities, and there seem to have been as many strikes and autonomous syndicates as there were forest regions) testifies to its origins. The lumbermen were no more artisans than were the harvesters; they were journeymen, working in the local farms when they could find work, especially in summer. As a local senator explained, "le bûcheron, c'est l'ouvrier rural, c'est l'ouvrier agricole."⁶⁶

In those parts of central France where farmland and forest are closely interlaced, most small farmers and day laborers made ends meet by seeking forest jobs during the winter season. Since the last years of the Empire, the local landlords who employed them had given way to urban lumber merchants. Harder times and growing competition among dealers led to deteriorating working conditions and growing friction between employers and their peasant work-gangs. Workers' grievances opened their ears to Boulangist arguments, and they were the first to unionize and strike in the early 1890s. They struck again in 1898–99, in the context of a would-be "national" movement. By that time, their ideas, carried out of the forest by fellow agricultural laborers, had affected sharecroppers and farmers in nearby areas. Leaving aside the original impact of Boulangist agitation, the important thing here is that the syndicalist contagion spread over the countryside. Syndicates had existed before—some founded by landowners and large farmers, others by or for urban workingmen—but peasants previously avoided them. As one peasant syndicalist explained, "landsmen and urbanites or villagers reciprocally ignore each other; the peasants obstinately refuse to join organizations founded by the workers of *bourgs* and towns; the socialist papers of Montluçon disapprove of [the peasant syndicates] and [the peasants] feel that the CGT approaches things from the urban workers' point of view."⁶⁷

Thus in 1900, almost a decade after the lumbermen had first tried and failed, the peasant-sharecroppers of Bourbonnais set up their own syndicates, the first of them at Gennetines and at Chézy (Allier), "unrelated to political or religious inspiration." Neither of these ventures was very successful; by 1909 or 1910, most of them had collapsed or lingered on only in the guise of reading societies or buying cooperatives. Still, despite their deliberate attempt to stay away from politics, they

⁶⁶ Dunois, "Le Mouvement Bûcheron," *Cahiers nivernais* (March 1909), 13; and Senator Girault of the Cher, speech of December 21, 1891, as quoted in Dunois, "Le Mouvement Bûcheron," 8. For this paragraph in general, see Dunois, "Le Mouvement Bûcheron," *passim*; Roblin, *Les Bûcherons du Cher et de la Nièvre*, 281, *passim*; and Pataut, *Sociologie électorale de la Nièvre*, 42. For an unsympathetic but sober account of rural syndicalism, see Auguste Souchon, *La Crise de la main d'œuvre agricole en France* (Paris, 1914), 111, 203–86.

⁶⁷ Jacques Chevalier, "Chez les paysans du Centre," *Revue catholique des églises* (November 1907), 523–26.

helped initiate their members—and their neighbors—to political life. Their direct economic impact was slight, but their efforts to mobilize parliament, government, administration, and the national press reflected their dependence on national authorities and spread awareness of politics as something that went far beyond local resolution of issues. At the same time, when the syndicates collapsed, they left behind a new sympathy for socialism, whose representatives had defended them in parliament. In Allier, where the sharecroppers' syndicates had been active though short-lived, Socialists made strong gains in the 1910 elections and retained their rural support in 1914. One historian of the Bourbonnais has attributed this support to the similarity the peasant found in socialist and syndicalist aims and hopes and the absence of dues and public commitment (hence, possibly, jeopardy) attached to socialist allegiance.⁶⁸ By the twentieth century, electoral politics had the distinct advantage not only of costing less but also of offering an opportunity to exercise one's will (that is, the right to vote) without letting on what this will was (that is, for whom one voted).

THIS CONCLUSION SUGGESTS THAT, before World War I, the pressures and conformities of country living inflected political activities there. Nevertheless, political life in the country was more active and better recognized than it had been a generation or two earlier. Obviously, this evolution was gradual and sporadic, appearing in different places to different degrees. The historian's problem is to follow it.

Some political scientists have suggested that abstentions from voting can furnish a rough index of politicization. But abstentions are just as likely to furnish indications of commitment and discrimination. Take, for example, the case of Louis Olivier, a bailiff of Lesneven (Finistère), who declared to a parliamentary commission that he paid no attention to the campaign of 1897, "having no candidate of his opinion," and did not even go out on the day of the vote.⁶⁹ Olivier was a sheriff's officer in a small town, but rural communities, though less articulate, appear to have shared his views. There, as in urban districts, the abstention rate was markedly higher when the vote was a foregone conclusion; and it was especially high when many voters could not find a candidate of their political stripe. Even under the Second Empire certain White royalist parishes in Vendée, villages that had taken an active part in the risings of the 1790s, showed two and three times the abstention rate of their neighbors. They were no more isolated than neighboring villages but had no Legitimist to vote for; rather than vote for a Bonapartist, they abstained. At La Réole (Gironde), which was equally rural, over 37 percent of the

⁶⁸ *Ibid.*; and Camille Gagnon, *Histoire du métayage en Bourbonnais depuis 1789* (Paris, 1920), 102–05. Also see Pierre de Fraix de Figon, *Le Métayage en Bourbonnais du point de vue économique et social* (Paris, 1911), 223. Writing in 1903, Roblin remarked that, in his day, in the Nièvre "political distinctions [were] just beginning to be grasped, thanks to the influence of the syndicates"; *Les Bûcherons du Cher et de la Nièvre*, 40–41.

⁶⁹ Finistère, March 1897, AN, C 5573. Since the contest was between the abbé Gayraud, a Red priest, and the Comte de Blois, a conservative Monarchist, one may assume that M. Olivier was a sound Republican. Also see the knowledgeable views of Charles Seignobos concerning abstentions in his *L'Évolution de la 3^e République* (Paris, 1912), *passim*.

electorate abstained in 1871, and over 40 percent in 1873. Then, in 1874, a Bonapartist candidate dared to show his head, and abstentions fell to 21 percent. By 1877, when political excitement was at its height, abstentions had fallen to 14 percent.⁷⁰ If abstention is an index of political apathy, it sometimes seems to have functioned *a contrario*!

Though equally impressionistic, I prefer the evidence found in the investigation of parliamentary commissions inquiring into allegations of electoral fraud, because it goes beyond the outside observer's judgment with which students of a popular and often illiterate public are condemned to content themselves. These files contain testimony from witnesses whose political observations are seldom otherwise recorded. What the files reveal is an evolution that was uneven, certainly, but palpable; the peasants, once granted the vote, gradually learned to treat it just as their enfranchised predecessors had done under the *monarchie censitaire*: as a source of personal and communal advantages. As early as 1847 Duvergier de Hauranne reported their astute observations: "Why shouldn't we sell ourselves for money, when the bourgeois sell themselves for jobs?"⁷¹ Once they began to sell themselves for money, they came to recognize the possibility of more profitable returns.

But that took time. Threats, menaces, bribery and corruption of various kinds, or simply forcing electors to vote correctly had all been common under the Restoration and the July Monarchy. They continued to be so under the Second Empire (and probably under the Second Republic before it), despite the decree of 1852, which provided that those convicted of intimidating electors into abstaining or casting an unwilling vote were subject to a minimum of one year in prison and a large fine. This decree hardly seems to have affected the spirit of electoral campaigning, especially in rural areas, where by the 1860s fraud was taken for granted by all. In some villages, mayors avoided even the pretense (and bother) of balloting, confining themselves "to certifying the presumed operations" and the desired result.⁷² More often, however, mayors simply substituted ballots of the candidate they favored or counted the votes as they pleased. Complaints were prevalent that more votes were counted than had been cast—Finistère, in 1869, for example: at Lacumolé, 256 electors but 264 votes; at Trévous, 285 electors but 304 votes; and at Arzano, 442 electors but 462 votes—or that votes that had been cast were not counted or were counted only for the official candidate—for example, in 1863 at Corcone (Gard) 24 cast but only 12 counted; in 1869 at St.-Radégonde (Gironde), 69 cast but only 23 counted; that same year at Montbéraud (Haute-Garonne), 41 cast but only 5 counted.⁷³ When such complaints were raised, officials ignored them, and government spokesmen dismissed them as normal corruption. Twice the minister refused comment, arguing that discussion would be contrary to the *secret du scrutin*.⁷⁴

⁷⁰ Fauchaux, "La Vendée," 148, 152–53; and Arambourou, *L'Arrondissement de La Réole*, 88.

⁷¹ *Moniteur universel*, 1847, p. 574.

⁷² See Rémusat, *Mémoires de ma vie*, 47–48. Also see Grenoble, August 16, 1864, AN, BB 1717.

⁷³ Montpellier, January 1865, AN, BB 18 1685; *Moniteur universel*, December 11, 1869, p. 1509 (Gironde); *ibid.*, December 25, 1869, p. 1608 (Finistère); *ibid.*, p. 1609 (Haute-Garonne); and *ibid.*, March 21, 1863, p. 299 (Gard).

⁷⁴ At Chézan (Hérault) the ballot box held thirty more bulletins than there had been voters; the magistrate at Béziers decided to do nothing; Montpellier, January 30, 1866, AN, BB 1717. In the first district of Marne, a

Alexandre Pilenco, the first student of electoral fraud in France, concluded that direct administrative intervention of this sort became extremely rare after the 1880s. Perhaps. At any rate, in the opening years of the Third Republic rural societies were still subject to the bonds of economic dependence and social deference and to straightforward intimidation by national or local authorities. In 1876 and 1877, for example, police confiscated copies of the *Dépêche de Toulouse* in Luchon marketplace and frightened off those carrying the paper; employers on both sides often expected their workers to vote for the candidate they favored, although not all went so far as the gentleman of Pontivy who circulated an official notice to that effect;⁷⁵ and victory *farandoles* and *charivaris* enlisted traditional forms in the service of novel activities that were guaranteed to confirm and deepen the divisions that politics espoused. In Brittany, but not only in Brittany, references to widespread unfamiliarity with the French language and also to illiteracy were abundant. At St.-Barthélémy (Morbihan) the mayor sought to mislead illiterate peasants, distributing ballots to them under false pretences: the mayor gone, "the woman, Le Nedelec, who could read, saw that M. le Maire had deceived them." There were numerous allegations of illiterate voters taking the ballot of one candidate for that of another or being gulled into voting for a man other than their own.⁷⁶ And there were still more cases of outright interference with electors—mayors, priests, or notables thrusting ballots of their own candidates into electors' hands, opening folded ballots, taking ballots away from unwilling voters, throwing ballots away, or simply refusing to accept legitimate ballots.

Such practices seldom reappear after 1877 outside the most backward regions—like Morbihan, where, as late as 1902, priests and landowners retained their age-old power and where laborers and domestics were marched to the polls, each holding his ballot in one *raised* hand.⁷⁷ More generally, however, straightforward brutality had given way to ruse or to more familiar forms of seduction. The first references I have found to buying votes date from 1862 and 1863. They testify to a new

vast variety of frauds were admitted; the government spokesman described them, however, as unremarkable—what happened there, he said, happened in a hundred other elections; *Moniteur universel*, March 17, 1863, pp. 280–82. For ministerial lack of comment, see *Moniteur universel*, December 11, 25, 1866, pp. 1510, 1612. At least the minister was consistent!

⁷⁵ Pilenco, *Les Mœurs du suffrage universel*, 176–77. Also see Sherman Kent, *French Electoral Procedure under Louis Philippe* (New Haven, 1937), and *The Election of 1827 in France* (Cambridge, Mass., 1975). For a detailed account of electoral maneuvers and fraud at the village level, see Sylvestre, *Un Scandale électoral: Rapport au Comité Républicain Libéral de l'Ardèche sur les élections municipales de Balazuc* (Aubenas, 1909). The great change came with the introduction of the secret ballot, first used in the elections of 1914. For Luchon, see 2d district St.-Gaudens (Haute-Garonne), 1876, AN, C 3158; and, for Pontivy, see Pontivy (Morbihan), 1876, AN, C 3159. The Pontivy circular is worth quoting: "M. le Vte de Kergariou fait savoir aux fermiers de Mr. de Lausanne, de Mme de Guélen et de Mlle de Coussin qu'il est chargé par les propriétaires ses parents de faire voter sans exception tous les fermiers pour Mr. le Comte de Mun. Il doit s'assurer de la façon dont cet ordre sera exécuté pour en rendre compte à leurs propriétaires qui sauront s'en souvenir." To demonstrate that nothing had changed by the elections of 1881, see AN, C 3323.

⁷⁶ For St. Barthélémy, see Pontivy, 1876, AN, C 3159. For all other allegations, see Pontivy, 1881, AN, C 3323; 2d district Poitiers, 1881, *ibid.*, 3324; and Beaulieu (Corrèze), 1893, *ibid.*, 5573. For an earlier instance at Ballons (Drôme), see Grenoble, August 16, 1864, AN, BB 1717.

⁷⁷ Lorient, 1902, AN, C 7306. But Roblin described the voting procedures of one of his lumbermen at the turn of the century: "He arrives at the *mairie* with his ballot prepared and folded. If he puts in the box a name the mayor wouldn't like, he writes (or has someone write) the name on the ballot paper of the other candidate. And these precautions are not useless in small places!" *Les Bûcherons du Cher et de la Nièvre*, 41.

sharpness of electoral contention, which official pressures alone could not resolve. Some peasants attained a measure of autonomy and decision that a generation earlier only their betters enjoyed. Obviously, menace was more economical than purchase.⁷⁸ The sale of a vote is no warranty of political awareness, but it does suggest that the voter had some freedom to choose and that a vote had value—a notion capable of translation to another plane.

The cash value of votes varied. Precise figures are seldom given, but those I have seen suggest that in the 1860s votes ran between ten and twenty centimes in Brittany, while in the Marne “two francs per vote is not expensive but more than is necessary.” By 1881 at Pontivy (Morbihan), where many votes had been bought for three to ten francs each, and where whole Republican villages were alleged to have been turned around, the subprefect expressed surprise at the low cost of political loyalty in the region. Generally, the price of votes seems to have reflected the bitterness of electoral combat, so that in 1869, in the hotly disputed first district of the Pyrénées-Orientales, men were getting up to ten francs a vote; and peasants did not take long to realize the possibilities of such situations. “They offered me ten francs,” said one witness. “I refused. Then my son said to me, Take it anyway. You’ll vote as you please.”⁷⁹

During the 1880s and 1890s, the practice of treating and “corrupting” electors gained ground. The poorer the region, the more prevalent the problem. In the legendary elections of the first district of Brive (Corrèze) in 1893, the rich Mielvaque de Lacour, whose platform had no political coloring whatever, did best among poor villages of the *châtaigneraies*, his Republican opponent in the more prosperous wine-growing areas.⁸⁰ Yet it would be wrong to regard bribery and corruption as limited to poor and backward regions. The Alps and Pyrenees head the lists, but the records show that such practices were rife before 1902, and to some extent up to 1914, in departments from Aude to Manche to Pas-de-Calais, from Dordogne and Ardèche to Vienne and Seine-et-Oise. They were only curbed by the law of March 31, 1914, which prohibits gifts in money or kind as well as promises of

⁷⁸ Riom, April 14, 1862, AN, BB 18 1632; and *Moniteur universel*, March 17, 1863, p. 280. Rémusat noted the change. In the elections of 1863, he spent about four thousand francs. “It was a lot compared with the elections of the past, which did not cost one hundred francs. It was little besides what elections would henceforth cost.” Thereafter, the average election cost him twelve thousand francs; but others later spent thirty-five thousand francs and more. “The invasion of elections by democracy has handed France over to wealth.” *Mémoires de ma vie*, 161.

⁷⁹ For Brittany, see Rennes, March 12, 1864, AN, BB 1688; for Marne, see *Moniteur universel*, March 17, 1863, p. 280; for Pontivy, see Sous-préfet, Pontivy, AN, C 3323; and, for Pyrénées-Orientales, see *Moniteur universel*, December 9, 1869, p. 1493. The subprefect’s surprise reflects more his reaction to such monetary transactions than expert judgment since, up to the elections of 1910, as Pilenco has shown, ten sous (50 centimes) is the figure most often quoted, with a low in the Doubs, where in 1906 a vote could still be had for two sous; *Les Mœurs du suffrage universel*, 213.

⁸⁰ *Rapport Dejean*, 1893, AN, C 5572. A more sophisticated form of the same type of electoral blandishments could be found in those districts wooed by candidates whose chief title was their wealth. That same year, 1893, almost all witnesses in the investigation of Edmond Blanc’s election at Bagnères (Haute-Pyrénées) denied straightforward corruption or fraud. They could, of course, be lying. Electoral Commission Report, Bagnères, 1893, AN, C 5574. But one can easily understand how many inhabitants of a poor district might expect to benefit from the election of a rich and helpful representative, even in the absence of personal gratifications. Indeed, a decade later, following the elections of 1902, an anonymous letter was written that denounces Paul Truy, elected at Montreuil-sur-Mer (Pas-de-Calais) for *not* being the millionaire banker that he had claimed to be and that people had supported. Electoral Commission Report, Montreuil-sur-Mer, 1902, AN, C 7305.

favors or jobs. Until then, open bribery continued to be evident, particularly in Brittany, where the economic dominion of great landholding families by the twentieth century seems to have required the support of cash and copious libations. The cost of Breton elections was notorious, and the quadrennial inflow of petty cash (and the outflow of tipple) came to be relied on as a contribution to the local economy. At Kermoroch (Côtes-du-Nord) "the commune" supposedly "belonged almost entirely to M. le Duc des Cars." Yet the "influential" duke's "influential" steward had to pay handsomely for his peasants' votes.⁸¹

Such customs dated back at least to the 1880s—testimony, again, less to the generosity of Breton magnates than to the bitterness of some electoral conflicts and to the venality of the peasants. As an old electoral hand testified in 1902, "Our Bretons quickly turned the exception into a precedent . . . , made it into a tradition; since then, electoral campaign signifies drink and money to them." It may be that carousing had preceded pelf, especially since Brittany took its time to enter a money economy. In 1869, it seems, "cider and the pope sufficed to gain all of the votes of Morbihan."⁸² At any rate, I am inclined to hazard that, there as elsewhere, payment in species rather than tipple came later, occurred more rarely, and disappeared sooner.⁸³ Drinks, however, had to be offered even if a candidate was running unopposed: "a dry election" drew no voters. No wonder a Pyrenean inquiry concluded that "electors have to walk or ride five or six miles to attend a meeting. How could you send them back without a drink?" On the whole, a philosophical attitude prevailed. As the reporter for one parliamentary commission declared, "In conformity with Breton electoral custom, [the voters] drank with M. Brune and they drank with M. La Chambre; the parties are even in their alcoholic largess. . . . While blaming intemperance, the members of this commission do not consider themselves empowered to reform the mores of the department of Ille-et-Vilaine."⁸⁴

Even so, there are scattered indications that a new sense of civic dignity was emerging. Lies and dissimulation had long been the nonconforming peasant's only recourse, whether against his fellows or against his betters. Centuries of experience had perfected attitudes that could be adjusted to electoral situations as they

⁸¹ For Kermoroch (and other nearby localities), see 1st district Guingamp (Côtes-du-Nord), 1906, AN, C 7305. Protesters claimed that the successful candidate had spent between fifty and sixty thousand francs to buy electors. At Quimper, 1st district, "not a very demanding region until then," bribery and drink in large quantities were denounced as new "Boulangist methods" in 1902. By 1906, at Quimper, 2d district, Republicans estimated that their unsuccessful opponent had spent two hundred and fifty thousand francs in four months of campaigning, during which "the age of gold returned to earth"; Georges LeBail, *Une Élection législative en 1906* (Paris, 1908), 84, 141, 146.

⁸² 1st district Guingamp, 1902, AN, C 7305; and LaCombe, *Profilis parlementaires* (rev. edn.), 152.

⁸³ In 1866, a man condemned at Bonneville (Haute-Savoie) for having given or promised money in exchange for votes explained that his political opponents had provided the peasants with drink. Since he found no inn in one village, he gave the peasants money instead. Chambéry, August 3, 1866, AN, BB 1692.

⁸⁴ 2d district Pau, 1902, AN, C 7375; and Robert Mitchell, rapporteur, 1st district, Saint-Malo, in *Journal Officiel*, December 19, 1889. Also see Chambéry, 1902, AN, C 7305. The notion that voters should not be allowed to go home without a drink occurs in almost the same words for the elections four years later; LeBail, *Une Élection législative en 1906*, 151. Here must lie the origins of the Pyrenean *rastell*, first introduced by Whites (*carlis*) in a cantonal election of 1864, when they offered drinks in a stable before the horse-troughs (*râteliers*). "This shameful custom [soon came to seem] natural to the natives," as the prefect of the Pyrénées-Orientales complained in 1867, and caused "enormous expense." Horace Chauvet, *La Politique roussillonnaise de 1870 à nos jours* (Perpignan, 1934), 79, and *Histoire du parti républicain*, 189.

appeared. Folktales reflect the yokel's ambition to trick the devil or any of the devil's surrogates: city folk, social superiors, or, simply, enemies. The peasants had no problem about "singing the right song" and doing as they pleased, when the occasion offered.⁸⁵ Gradually, however, democratic propaganda spread abroad the sense that there was something wrong about the very need for subterfuge. Free men should vote freely, and their votes should count and should be counted. When, in 1869, the results of the Haute-Garonne elections were skewed by heavy fraud and official intervention, the peasants clearly understood that their opponents had not *won* their victory, but *stolen* it.⁸⁶

Interest and pride are hard to disentangle. Dressing in Sunday best to cast your ballot is a small indication of pride. But, as one rural representative wrote Thiers in 1873, "the peasants [had] understood the value of their vote," so that they began to exhibit "the sentiment of political freedom." Increasingly courted by betters who had long ignored their existence, let alone their significance, smallholders were conducting a small revolution of their own. Some time passed before they claimed mastery of their own territory, but their growing civic pride is unmistakable.⁸⁷ In September 1893, Audeguil Pierre of Saint-Antoine (Corrèze) testified that he had refused drinks offered by an electoral agent—"being not merchandise but a man." Another farmer sent a letter, written (in fine calligraphy and execrable spelling) "par mon petit qui va en clace," to tell the commission of inquiry "que je saient un homme qui et republiquin." Most revealing, perhaps, are the inscriptions traced on the walls of the extraordinary dream mansion built between 1879 and 1912 by a village postman in the Drôme. Ferdinand Cheval, of Hauterives, insisted that his feat was "the creation of a peasant" who gloried in his labor and found happiness in the honor of achievement. Creativeness, honor, glory, valiance, pride—once the preserve of their social superiors—were values to which peasants also could now aspire.⁸⁸ Seen in this perspective, the vote was but another component of a new self-awareness.

As more electors became interested in exercising their voting rights and as open repression or fraud became more difficult, personal corruption (buying or treating) became a symbol less of subjection than of emancipation. So, perhaps, did the violence of organized gangs hired to keep electors from the polls. The harder it became to buy or intimidate electors, the more important the transitional role of physical violence. With official violence on the wane, private enterprise entered the field. In 1893, in the first ward of Brive, several groups of toughs under the leadership of picturesque characters—Alfred Larue *dit l'Hercule*, Peuchaneil *dit*

⁸⁵ Henri Vincenot, *Le Billebaude* (Paris, 1978), 67. Also see Riom, April 14, 1862, AN, BB 18 1632; and Guillaumin to Gabriel Maurière, March 12, 1908, in Roger Mathé, ed., *Cent dix-neuf lettres d'Émile Guillaumin* (Paris, 1969), 76.

⁸⁶ For the concept of free men, see Agen, January 9, 1869, AN, BB 389; and, for the stolen victory in 1869, see Rémusat, *Mémoires de ma vie*, 249.

⁸⁷ Louis Latrade, of Corrèze, letter of May 7, 1873, as quoted in Daniel Halévy, *Le Courier M. Thiers* (Paris, 1921), 491–92. On the flattery of the electorate, see Barral, *Les Agrariens français de Meline à Pisani*, 39–40; and, on growing pride, see *Enquête sur l'agriculture française*, AD, Corrèze, 8°T² (13); and Fernand Giraudeau, *Bleus, blancs, rouges* (Paris, 1873), 187.

⁸⁸ Letters of Pierre and Joseph Charpentie, in 1st district Brive, AN, C 5573. Short of a visit to the monument (or "folly") in Hauterives, the most accessible information lies in Michel Friedman, *Les Secrets du Facteur Cheval* (Paris, 1977).

Barbe de Fer, *Aubignac dit le Zouave*—broke up meetings and discouraged the faint of heart. A decade later, lower Brittany became sufficiently emancipated to follow suit. In 1903, at Plougoumelen and Biedz, eight members of hired sailor gangs were arrested and tried for terrorizing the electors.⁸⁹

Other aspects of electoral campaigns also evolved. In the 1870s it was still possible to incite panic among what one witness described as “decent but credulous” communities with false news of rampant revolution: the Midi in flames, red flags flying over ruined churches, and massacred priests. At Boulloc (Haute-Garonne) the Legitimist-Bonapartists promised that if the Republicans won priests would be defrocked, private property would be divided, churches would be torn down, and society would be lost. At Massouins (Var) the priest warned his flock that after a Republican victory there would be no more burials in holy ground and no baptisms of infants and there would be damnation for those who had voted Republican. At Pontivy in 1881, circulars in the Breton tongue took “advantage of the intellectual and material isolation of the Breton people” by announcing the return of the Comte de Chambord and the approaching end of the Republic. In the second district of Poitiers (Vienne), the Royalist candidate was denounced as standing for the guillotine, bloodshed, and Henry V; the Republican as a warmonger whose success would mean mobilization for all men to the age of forty. In some places it was asserted that war had already been declared and mobilization begun. The false rumors appealed to the well-established rural phobia against military service but also rested on the untimely coincidence that men in the army reserve were just being called up for their twenty-eight-day obligatory service. Yet even while threats of war were being bandied about, new themes could be heard in scattered villages. At Civray and Availles, candidates argued about education, Republicans promising free school for all and anti-Republicans predicting prison for parents who would not send their children to school.⁹⁰

Michael Burns’s current work demonstrates how the Dreyfus case with its national themes—like antimilitarism and support for the army, antisemitism and nationalism—and personalities—like Émile Zola—entered the consciousness of small rural communes, affecting their rituals (during Carnival or draft ceremonies), their imprecations, and their propitiatory rites. Antisemitic posters displayed at fairs linked Jews to usury, grain speculation, and engrossing. Rural Gers returned three antisemites to the Chamber in 1898. That same year the mayor of a small village, Baigneux, in Loir-et-Cher attributed the agricultural crisis to “the wild speculations of the cosmopolitan Jewish gang.”⁹¹ In the recent past, inexplicable troubles had been ascribed to supernatural forces or to the dark machinations of landlords and governments; now they were explained by city-forged formulae.

⁸⁹ 1st district Brive, 1893, AN, C 5572, 5573; and *Journal Officiel*, 1903, p. 1165, as quoted in Pilenco, *Les Mœurs du suffrage universel*, 255.

⁹⁰ Haute-Garonne, 1876, AN, C 3158; 3d district Puget-Théniers (Basses-Alpes), 1877, *ibid.*, 3229; Pontivy, 1881, *ibid.*, 3323; 2d district Poitiers, 1881, *ibid.*, 3324; Civray, 1881, *ibid.*; Availles, 1881, *ibid.*; J.-P. Charnay, *Le Suffrage politique en France* (Paris, 1965), 322; and LeBail, *Une Élection législative en 1906*.

⁹¹ Lacoïn, *Enquête sur la crise agricole*, 43–44. Also see Stephen Wilson, “The Antisemitic Riots of 1898 in France,” *Historical Journal*, 14 (1973): 789–806.

"RURAL CREDULOUSNESS" APPEARS IN THE SOURCES through the 1880s but I have found no reference thereafter. Wider perceptions spawned novel allegations. A witness from Morbihan in an inquiry of 1902 declared that he was called a *dreyfusard*, "which is the worst insult in our regions." Here the Dreyfus affair evidently symbolized the diabolical forces that menaced decent people and the Church. During the 1893 elections in Ardèche, a Republican candidate was "exorcised" by a Jesuit priest on the grounds that he was possessed by the devil. His opponent, Eugène-Melchior de Vogüé, was much hurt by reminders that, during 1846–48, his grandfather "avait noyé le blé pour affamer le peuple"—had speculated on the price of grain, a painful memory still alive in many peasant homes. Yet in the same election Vogüé referred (apparently with some effect) to his role in establishing the Franco-Russian alliance, and his opponent felt that this "exercised considerable action on people's minds."⁹²

Justified or not, this sort of campaigning suggests a widening of local horizons and a new awareness of factors to which the peasants had once been indifferent: urban myths, foreign relations, or tariff policy. Countryfolk continued to be like hedgehogs, suspicious of wide-roving foxes, but the hedges they ranged now ran far afield. Even folktales reflected this new awareness of a wider world and the entry of politics into everyday village life. Another "we" appeared, another *pays* took shape; and both were larger. So was their context. The impact of imported grain upon domestic grain prices also widened the horizons of villagers. As early as 1881, a villager at Genouillé (Vienne) criticized a candidate "for having opposed the entrance of American grain into France, thus making ours rise to forty francs." When surveyed in 1898 the village mayors of Eure-et-Loir attributed their economic difficulties variously to insufficient tariff protection, imports from Russia, Germany, and the United States (especially wheat), the rapid development of backward countries, and French reluctance to abandon outmoded production.⁹³ Admittedly, these were notables—however minor—in a relatively well-situated department. But the same year, a poor farmer's son from Ygrande (Allier) could write a dialogue, "Le Blé d'Amérique," for the local paper. A few years later, the same Émile Guillaumin, now an established writer though a farmer still, published a novel in which a peasant discussed crops and prices with two passing tramps who knew a great deal about national and international conditions: "Bah!" said one of the tramps, "it's not because the crop in central France has been a bit below average that you can expect higher prices; we got a good yield from the North and West; and from America and Russia one can get all one wants." "And from Algeria . . .," said the vagabond Misery, who had served there.⁹⁴

⁹² On credulousness, see Gien (Loiret), 1889, AN, C 5468; and Morbihan, Procès-verbal, July 9, 1902, *ibid.*, 7306. Also see Eugen Weber, *Satan, franc-maçon* (Paris, 1964). For the Ardèche elections, see 2d district Tournon (Ardèche), AN, C 5572.

⁹³ Charles Joisten, *Contes populaires du Dauphiné* (Grenoble, 1971): 264, 274–77; and Civray (Vienne), 1881, AN, C 3324. Also see 2d district Poitiers, 1881, AN, C 3324; and Lacoïn, *Enquête sur la crise agricole*, 23, 24, 25, 32, 34, 87, 105.

⁹⁴ Guillaumin, *Albert Manceau, Adjudant* (Paris, 1906), 9, and "Le Blé d'Amérique," reprinted in his *Dialogues bourbonnais* (Moulins, 1899): "Ah j' sais ben! leu Mérique! Ca fait rien, quand y aura pus d'farine pour faire du pain, avec leu Mérique j' pense pas qu'o pussient faire quéque chouse de bon."

Adult access to information, through printed matter, grew notably in the 1880s and 1890s with the education of their young. The father of another of Guillaumin's characters, an illiterate farmer, subscribed to two papers "as soon as [his son] was able to read properly." "Quelle joie pour lui de se faire lire le journal!" Of course, many (perhaps most) peasants still could not understand those interested in the news the paper brought: "what did not affect them directly remained strange, distant, suspect. . . . 'As if for people like us that means anything,' they said."⁹⁵

But even villagers realized the concrete importance of "governmental favors": "to have better roads, to obtain a dispensation from or a postponement of military service for a son, to get a relative hired in an administrative service or a *bureau de tabac* for oneself, to receive compensation for crops ravaged by hail or cattle decimated by disease, or simply to avoid official vexations—here are powerful reasons . . . to vote for the governmental candidate and to show the greatest zeal in supporting his ideas," wrote a historian of poor, back-country Cantal.⁹⁶ That was precisely the criterion for electoral support established by bourgeois electors of the 1840s: "our roads, our railways, the taxes, the upkeep of [public] buildings—what have you done for them?"⁹⁷ In nearby Lot, still in 1905, "our peasants belong to whoever gives them a living." This had become the motive that led them to shift from Bonapartist to Republican in 1889. They "have got into the habit of being with the government and receiving its favors." They may have learned that lesson sooner (they had done so in Auvergne) from migrants who disseminated the conclusions they had picked up in the hard school of Paris: to serve one's home interests, one "has to use the only weapon that is effective today, the political weapon."⁹⁸

The old-fashioned patron-client relationship continued, but the actors had changed. The leading roles were no longer held by local notables in a position to provide work or help in need. Their place had been taken by those best able to secure governmental intervention. As Émile Guillaumin wrote grudgingly to his sharecropper-readers, "Despite its vices and its shortcomings, one has to admit that the make-up of parliament affects our daily life. That certain politicians have

⁹⁵ Guillaumin, *Le Syndicat de Baugnoux* (Paris, 1912), 6, 14, 42. Compare the edifying image of newspaper reading offered in Paul Bert, *L'Instruction civique et morale à l'école* (Paris, 1881), 67–70.

⁹⁶ Rouchy, "St. Flour," 550–51. This sometimes produced unpredictable results, as in the Jura, particularly well served by its deputies, which was left with a thick but costly network of narrow-gauge railroads that were completed on the eve of the auto era. Worse still, the influence of Georges Trouillot, who represented Beaufort canton, had got most local youths posted for military service to the closest garrisons, Lons-le-Saunier and Bourg, whose units were especially hard hit in 1914. Beaufort was one of the French cantons with the most war dead. Prost, *Charles Dumont*, 44.

⁹⁷ Avocat, électeur, *Simple opinion sur les candidatures électorales de l'arrondissement de Troyes* (Troyes, 1846). Also see, for fascinating recollections of electoral politics under Louis-Philippe, *Moniteur universel*, January 12, 1864, p. 54. Writing at the turn of the century, Georges Clemenceau, scion of an experienced political clan, noted as a matter of course that "local politics in general resolves itself as it does everywhere, into a question of clientele." But, he added suggestively, "the calculation is quickly made of what one vote for one side or the other is worth." Such freedom to calculate had not always been there. Clemenceau, *Figures de Vendée* (Paris, 1930), 117–18. For the view that the reduction to questions of clientele "often, still" happened, see *ibid.*, 235. Such testimony suggests that such happenings were less frequent and less commonplace than they once had been.

⁹⁸ Calvet, "Cahors," 77–78, 83; and *L'Auvergnat de Paris*, August 1882, as quoted in Raison-Jourde, *La Colonie*, 268. *L'Auvergnat de Paris* describes the use an Auvergnat leader, president of the Republican-Socialist committee in his district, made of politics: "S'il servait la politique avancée, c'était avec l'espoir même pas dissimulé qu'il s'en servirait à son tour ... pour ses oeuvres, pour son pays d'Auvergne." *Ibid.*, 267.

spoken about sharecropping in Bourbonnais is not a matter of indifference. It does not mean that things will change tomorrow, but it does mean that, if those concerned wanted to do so, they could change something more easily."⁹⁹

The peasants had accepted the Republic, had become republican, for reasons not unlike those that had made them accept the Second Empire: higher cattle prices, free schools, a shorter term of military service, old age allocations, and so forth.¹⁰⁰ More important, though, they had become aware that national and international events had an impact on them; that villagers could, however slightly, *affect* parliamentary politics and, thereby, their own lives; that their efforts, if they were willing to make them, could bring about *some* change. Here, surely, were new perceptions and possibilities that testify to a degree of politicization that is recognizably modern—that is, a political awareness not very different from our own.

⁹⁹ *Le Travailleur rural*, September 1910, reprinted in Émile Guillaumin, *Six ans de lutte syndicale* (reprint edn., Moulins, 1977), 93–99. Compare Joel S. Migdal, *Peasants, Politics, and Revolution* (Princeton, 1974), with the analysis presented here. Migdal has described the peasants of Coyotopec (Oaxaca), who, after ages of concern about their unruly river, finally sent a spokesman to Oaxaca to ask the government for a footbridge. And he has argued that “peasant participation in complex political organizations is realized in return for material inducements.” *Ibid.*, 4–5. Although I grant his point, I disagree with the miserabilist coloring of the process he has described. His peasants are “overwhelmed”; they are “victims”; they are “forced into an entirely new world”; they are no longer “protected” by “old modes of life and action”; and their “entry into the wider world” is “painful”; *ibid.*, 257–60. This is not my impression of what happened, nor was it that of the Breton peasants; see, for example, Burguière, *Bretons de Plozévet*.

¹⁰⁰ See Émile Guillaumin to Georges Valois, October 15, 1908, in Mathé, *Cent dix-neuf lettres*, 82.

AHR Forum

Insiders and Outsiders in American Historical Narrative and American History

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THIS ESSAY HAD ITS ORIGIN in a slightly frivolous but nagging question that a previous research project had left unanswered. In a monograph about American spiritualism, I had made the assumption often associated with scholars who pursue American Studies that no historical belief or activity can be wholly deviant with respect to the age in which it appeared. Everything, after all, is a product of its cultural milieu and, therefore, has some more or less normal meaning within the culture. That assumption, among other things, steered me away from the tendency evident in some earlier historical accounts to interpret nineteenth-century spiritualists as either “kooks” or charlatans. Spiritualists, it clearly appeared, had given expression to many central intellectual currents of their time. For that reason, millions of Americans in all social classes had taken a strong and sympathetic interest in their claims. Arguing that American spiritualist leaders were close to the center of the American belief structure (they were “insiders”), the book insisted, I suppose, on interpreting American psychics as part of the mainstream.¹

Despite the book’s demonstration of the importance and prevalence of spiritualist belief in nineteenth-century America, however, considerable evidence remained to suggest that the argument that rendered spiritualist perspectives “normal” or “typical” was downright perverse. Anyone who has read the writings of the leading proponents of spiritualist belief—or, for that matter, of the abolitionists and feminists with whom they frequently allied—is aware of their heavy reliance on what can appropriately be called a rhetoric of deviance.² In their public and private statements, they constantly claimed that they were outcasts (or “outsiders”). Since they had good and tangible reasons to talk about themselves as the victims of verbal and physical abuse, I had to ask myself whether efforts by the historian “to mainstream” spiritualist belief distorted the way in which historical actors perceived

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¹ Moore, *In Search of White Crows: Spiritualism, Parapsychology, and American Culture* (New York, 1977).

² *Ibid.*, 90–101. For a discussion of the alliance of spiritualism and feminism, see William Leach, *True Love and Perfect Union: The Feminist Reform of Sex and Society* (New York, 1980), esp. 292–97.

themselves or sought to resolve in too simple a way ambiguities inherent in their self-perceptions. A reading of the “objective facts” about the popularity of spiritualist activities seemed to bear an antagonistic, certainly confusing, relationship to any number of realities vigorously asserted by all of the parties in the controversy. Were spiritualists inside the nineteenth-century mainstream (because they created a broad popular movement) or outside it (because they constantly emphasized the powerful opposition they aroused)?

This nagging question would remain frivolous if the only thing at stake were the proper interpretation of American spiritualism. More general problems, however, are involved in the questions we pose about insiders and outsiders in America's past and present. American historical narrative has depended, perhaps to an unusual degree, on tales about people identified by one of these two labels or their equivalents. Given the “Balkanization” of the American past evident in our recent narratives, that dependence may even be increasing. In a period when the ideas of national character, an integrated society, and a shared national culture have come “under withering attack,” narrative all the more seems to need a center;³ and reference to a mainstream, our currently popular metaphor that automatically divides the world into insiders and outsiders, provides one. The boundaries of a particular mainstream may be broad and fluid, but the metaphor nonetheless leads historians to distinguish between the people in its central currents and those in side channels and backwaters.

When historians write about insiders and outsiders, about mainstreams and eddies, they are doing more than locating a center. Historical stories about insiders and outsiders are constructed from the implicit and explicit assumptions that historians make about how power and status have been distributed in American life, about how values have been created and disseminated in a plural society. Historians locate the mainstream according to how they conceptualize majority and minority groups and according to how they analyze such sociological categories as marginal and elite groups and sub- and dominant cultures. Their division of historical landscapes into insiders and outsiders, ingroups and outgroups, conceals a multitude of judgments about American socioeconomic structure.

Historians are, of course, supposed to disagree about these matters, and the point of the ensuing discussion does not mean to suggest how we might attain a steady definition in our narratives for terms like “typical” or “dominant.” Quite the contrary. Its point is to clarify some of the reasons why our disagreements about these matters have no ultimate resolution. It is also to suggest some ways in which we can make better sense of the historical contests between insiders and outsiders and to clarify the meaning of those contests within the context of American pluralism. To do these things, I will pay attention not only to the ways in which historians have used insider and outsider labels but also to the ways in which historical actors have used them. For both historians and historical actors often tend to conceal or neglect ambiguities that are essential to the meaning and importance of those and closely related forms of identification.

³ John Higham and Paul Conkin, eds., *New Directions in American Intellectual History* (Baltimore, 1979), xii.

The organization of this essay is perhaps sufficiently eccentric to warrant a few initial sentences about the direction and aim of the argument. First, I will try to clarify the ideological perspectives that often lurk behind historians' use of insider and outsider labeling and to show in what ways the most familiar patterns of American historical narrative stereotype the role of outsiders. In the next section, I will examine some examples of insider and outsider rhetoric used by historical figures and suggest that historians' stereotypes, though all derivable from the rhetoric, often neglect important counterimages, which are frequently suppressed in the rhetoric but are nevertheless essential to its meaning. The following, and longest, section discusses some common consequences of the neglect of ambiguities in outsider identity. What mistakes of historical judgment can result when counterimages are not given due weight? In what ways can the historian's estimate of the political (or social or economic or intellectual) significance (or insignificance) of various outsider groups encourage a misreading of the meaning of outsider rhetoric? What can be misleading about the outsider's estimate of his own deviance? That section, though restricted to illustrations drawn from my current research in American religious history, suggests the need to redress a general interpretive neglect of the strategical uses of insider and outsider rhetoric. The final section of the essay draws out the reasons why contests between insiders and outsiders have an importance in America's pluralist past that cannot be analyzed solely in terms of objective conditions that determine America's socioeconomic structure. In that concluding part, I shall have something to say about what is wrong with the way we often try to understand typical or dominant culture in America.

The nagging question I asked earlier about nineteenth-century American spiritualists is, in fact, badly posed because attempts to answer it cannot at the same time address what we must often distinguish as levels of perceived reality and levels of factual reality. The latter relates to conditions that do not change as a result of what people, past or present, say about them. What follows is an attempt to ask better questions.

WHAT COMMON ROLES HAVE INSIDERS AND OUTSIDERS generally played in twentieth-century historical narrative? The Progressive historians are the appropriate starting point for discussion. Strongly influenced by Marxist dialectical ideas, they were perhaps the first American historians to make outsider groups important to an interpretation of American life. Certainly they were the first to take much explicit interest in the sociostructural determinants of group conflict. In the histories of Charles A. Beard and Vernon Louis Parrington, the dominant, though not necessarily typical, culture in America was economically determined, reflecting the value system of the largest propertyholders. Since financiers and industrialists commanded vast reserves of economic and political power, the conservative norms imbedded in the dominant culture determined much of what happened in American life. But, thanks to the struggles of the economic underdogs, they did not determine everything. Inspired in different eras by such people as Thomas Paine, Andrew Jackson, William Jennings Bryan, and Eugene Debs, outsiders—the

humble folk who individually had no economic power—arose periodically to challenge the overlordship of the propertied comfortable classes.

Progressive patterns of narrative often present historical outsiders as both victims and heroes. Outsiders suffered from injustices imposed upon them by the insiders who controlled the mainstream institutions of economic and social power. Because of the circumstances of their lives—their low wages, the declining importance of their skills, their recent entry into Anglo-American culture—they could not take their “power, status, or sense of reality for granted.” They were not, however, defeated or unimportant. Despite their victimization, they bore a counterimage of the American mainstream, one ironically derived from a close reading of the egalitarian principles of the Declaration of Independence. “Outgroups,” to quote Robert Kelley, whose recent noneconomic structuring of American conflict nonetheless fits a Progressive pattern, were “the ones among whom the basic ideals of the democratic order have been most energetically agitated.”⁴ Underdogs never successfully overturned the economic base of class dominance, but they did manage to organize effectively, especially in the Democratic party. Their struggle to shift the mainstream from what it was at any time in the past closer to what they thought it was and is supposed to be has never ended.

A markedly different configuration of insiders and outsiders is apparent in the narratives of Consensus-minded American historians. In the fashions of Consensus history, economically privileged groups have not foisted the dominant American culture upon powerless outsiders. Rather, the dominant culture in America has always been the result of a widely shared liberal viewpoint that grew naturally from the historical circumstances that gave birth to the American republic. Louis Hartz, whose classic book turned Alexis de Tocqueville’s tyranny of the majority into Lockean unanimity, argued this point of view most persuasively.⁵ But the general tendency of all Consensus narrative is to turn the mainstream into a broad and almost irresistible current. No longer conceptualized as the private swimming hole of economic elites, the center becomes a public bath to which almost all people, regardless of ties to minority or special interest groups, gain entry. In historical narrative, these assumptions work in a decisive way to undercut the role of outsiders as heroes and alter as well the meaning and significance of their victimization.

Consensus narrative turns most Americans into insiders who experienced outsiderhood, if at all, as a temporary form of identification. Focusing on the ever-widening mainstream, it argues that Americans were essentially alike even when they talked as if they were unlike. True outsiders—that is, alienated malcontents and socially marginal people—were not harbingers of progressive change. They were threats. They were the ones who might suddenly rally around the political banner of a Senator Joseph McCarthy. The narrative use that Richard Hofstadter

⁴ Kelley, *The Cultural Pattern in American Politics: The First Century* (New York, 1979), 27. For a preliminary statement of his argument, also see his “Ideology and Political Culture from Jefferson to Nixon,” *AHR*, 82 (1977): 531–62; and see Geoffrey Blodgett, “Comment” on Kelley’s essay, *ibid.*, 566–67.

⁵ Hartz, *The Liberal Tradition in America: An Interpretation of American Political Thought since the Revolution* (New York, 1955). For a general discussion of Consensus history, see Bernard Sternsher, *Consensus, Conflict, and American Historians* (Bloomington, Ind., 1975).

made of the anti-intellectual and paranoid behavior of outgroups very much shaped the Consensus interpretation of “real” outsiderhood as a kind of social disease. Insofar as that form of identity gathered strength in any particular period, it disrupted the comity necessary to the stability of American pluralism. In Consensus narrative, the victimization of true outsiders turns them not into heroes but into villains of varying degrees of malevolence.⁶

Many American historians who began to write in the 1960s noted what they believed was a fundamental dilemma in Consensus narrative. On the one hand, with the significant exception of Daniel Boorstin's books, most Consensus history written in the 1950s had intended to make criticisms of the American liberal imagination.⁷ The existence of malcontented outsiders, whose status anxieties led them to support extremist politics, suggested the need to transcend the social attitudes accepted by the average insider American citizen. Unfortunately, and this is the other hand, Consensus narratives never specified how that might be done. The logic of the narratives seemed to legitimize the politics of compromisers and technicians who had neither personal reasons for challenging nor the necessary ideological perspectives to challenge the status quo. If the Consensus interpretation is right, American insiders had succeeded in making too many people share their hostility to ideological divisiveness. Neither the majority who shared their views nor the dissolute and sometimes disruptive remainder appeared likely to become the agents of truly progressive change.

Having made that observation, many young Radical historians began to take steps that restored heroic stature to historical outsiders. The task was not always easy, for the Radicals' perspective on the past was not nearly so sanguine as the one of the more Whiggish Progressives. Although “New Left” historians agreed with the Progressives that economic and social power, rather than a majoritarian viewpoint, had determined what was dominant culture in America's past, they emphasized more strongly the strength of that power. In fact, in one way that emphasis represented less a return to Progressive narrative than a reinforcement of the Consensus interpretation. Much like Hartz, Gabriel Kolko and Howard Zinn plotted narratives that showed liberal belief strangling the American political imagination.⁸ In their view, a numerically tiny governing class created the mainstream. Power elites exercised hegemony at any point in our past because they owned a disproportionate amount of the country's wealth, received a disproportionate share of the country's yearly income, and contributed a disproportionate

⁶ For examples of the kind of analysis I have in mind, see the essays in Daniel Bell, ed., *The Radical Right* (Garden City, N.Y., 1964); Richard Hofstadter, *Anti-Intellectualism in American Life* (New York, 1963), and *The Paranoid Style in American Politics and Other Essays* (New York, 1965); and Seymour Martin Lipset and Earl Raab, *The Politics of Unreason: Right-Wing Extremism in America, 1790–1970* (New York, 1970).

⁷ See David Potter, *People of Plenty: Economic Abundance and the American Character* (Chicago, 1954), 91–141; Richard Hofstadter, *The American Political Tradition and the Men who Made It* (New York, 1951), v–xi; and Hartz, *The Liberal Tradition in America*, 3–32.

⁸ Zinn wrote in his *Postwar America*, “All that I have said here supports the ‘consensus’ interpretation of American history which states, I believe, a profound truth about our society, that its progress and its political clashes have been kept within severe limits”; Zinn, *Postwar America, 1945–1971* (Indianapolis, 1973), xvi–xvii. Something of the same perspective informs Kolko's *The Triumph of Conservatism: A Reinterpretation of American History, 1900–1916* (New York, 1963) and James Weinstein's *The Corporate Ideal in the Liberal State, 1900–1918* (Boston, 1968).

number of members to the country's controlling institutions. Most other people in any historical era were simply "co-opted" into the mainstream and fell into a state of false consciousness. The mainstream side won by taming or splintering dissenting viewpoints in ways that nullified any real threat to the status quo.

Although citations to the work of C. Wright Mills and Antonio Gramsci certainly encouraged a reading of the past that diminished the political achievements that Progressive historians wanted to credit to historical outsiders, transforming what were once considered great victories into empty reforms that actually strengthened the economic position of power elites, recent Radical histories have focused more attention on outsiders than the narratives of any other school of American historiography have. Indeed, politically conscious outsiders—the ones whose struggles resulted in something that historians might regard as "a politically significant cultural achievement"⁹—became *the* subject of American history in the 1960s and 1970s, as scholars paid attention to many outgroups that the Progressive historians had barely noticed—feminists, blacks, native Americans, and immigrants with preindustrial values. The narrative assertion that some of these outgroups had maintained a value system at odds with the dominant culture underlines what is, after all, a crucial distinction between Consensus and Radical narratives. According to the New Left historians, America's hegemonic bourgeois culture did not arise naturally out of shared historical experiences. It was the fabrication of specific class interests. The efforts of outgroups to gain a critical awareness of their true interests were therefore terribly important. Those struggles gave the past whatever meaning it has for the present.¹⁰ When Radical historians structure narrative around a politics of struggle generated by oppressed people outside the mainstream, they are paying homage to the most durable legacy of Progressive historiography. The Radical historian's outsiders are almost always defeated heroes or victims. All the same, just as in Progressive history, they invariably carry the true version of the American dream in their heads.

Heroes, villains, victims—these roles, shaped in one way or another by historians' perspectives, are the common ones that outsiders play in twentieth-century historical narratives. Those narratives are not, of course, quite so formulaic as my discussion here might suggest. But it should at least clarify what kinds of underlying issues historians argue when they divide the historical landscape between insiders and outsiders. How do social values and norms get created? How and why do the values of one group get imposed on another group? Who benefits from what gets called "mainstream" values and who gets oppressed? Who in the society can effectively contest the "mainstream" norms? The ways in which historians use insiders and outsiders in their narratives invariably indicate their answers to these questions. That is to say, whether historians make the outsider John Brown a noble hero or a misguided victim or a disruptive villain depends in large part on their views about the working, or nonworking, of American society.

⁹ I have taken this phrase from Eugene Genovese. Review of Leon Litwack, *Been in the Storm So Long: The Aftermath of Slavery* (New York, 1979), in the *New York Times Book Review*, June 10, 1979, p. 3.

¹⁰ See Herbert G. Reid, "Introduction," in Reid, ed., *Up the Mainstream: A Critique of Ideology in American Politics and Everyday Life* (New York, 1974), 2–9.

The last of the usual roles that outsiders play in narrative is, in fact, a nonrole. Obviously, Consensus-minded historians, because of the inclusive nature of their mainstream, often simply omit some outgroups from any serious consideration. Unless an outsider group threatened to become disruptive, it can have no particular importance in historical narrative. True outsiderhood in the Consensus view often becomes synonymous with historical insignificance. For somewhat less obvious reasons, Progressive and Radical historians can be almost equally dismissive of some types of outgroups. Although their mainstream is less inclusive, or is not given the same “legitimate” status that Consensus narratives accord to it, they focus their attention on outsiders whose politics supported what the historian judges to be a “progressive” trend. Other outgroups they ignore or treat with harsh condescension.¹¹ In addition, then, to the presentist assumptions implicit in the act of typecasting outsiders as heroes, villains, or victims, historians of all persuasions sometimes simply push, for various reasons, many outsiders to the fringes and margins of narrative. If they cannot serve a political, or ideological point in narrative, outsiders are in danger of disappearing altogether from historical sight.

HISTORIANS DEFEND THEIR NARRATIVE TREATMENTS of outsiders by insisting that a particular treatment is faithful to the facts. In other words, some outsiders really are heroes (Emma Goldman), some really are paranoid (Whittaker Chambers), and some really are best left to sociologists and psychologists of deviance (the people who died with Jim Jones in Guyana). In defense of these claims, historians can get a lot of help from the historical actors in question. After all, historical outsiders usually saw themselves as fitting stereotypical roles of heroes or victims, though not, perhaps, of villains. And many defined themselves as social radicals, or as persecuted martyrs, or as alienated underdogs. But the trouble is that outsiders quite commonly assumed all of the identities at once. Historians have no trouble quoting John Brown in ways that show the famous abolitionist analyzing himself as a victim, a hero, or a crazed fool. Before quoting words that emphasize one identity at the expense of another, however, they have good reason to think about why outsider rhetoric—or, alternatively, insider rhetoric—allows such a variety of interpretations. The rhetoric is not simple, and seldom does it in any easy way provide confirmation of or negate the points that historians often want to make in their narratives.

A few examples of “typical” insider and outsider rhetoric suffice to make clear the nature of the interpretive problem. The first one, drawn from an essay by the Reverend S. M. Campbell written in 1867, is on one level the declaration of someone who perceived his own values to be the ones that dominated American society and culture. “This is a Christian Republic, our Christianity being of the Protestant type,” he wrote. “People who are not Christians, and people called

¹¹ As a small example of this point, Christopher Lasch picked the word “grotesque” to describe nineteenth-century Mormon theology and the words “outlandish” and “trumped up” to characterize the “pseudo-historical myth” of Mormons as the chosen people. By using this sort of language, Lasch has reduced their significance to an illustration of ideological stupidity in American culture. See Lasch, “The Mormon Utopia,” in his *The World of Nations: Reflections on American History, Politics, and Culture* (New York, 1973), 60–61.

Christians, but who are not Protestants, dwell among us; but they did not build this house. We have never shut our doors against them but if they come, they must take up with such accommodations as we have.”¹² Campbell’s statement is obvious grist for any historical mill seeking to make Protestantism the dominant American culture of the mid-nineteenth century. Campbell’s words imply that he, as a member of that dominant culture, took his status and identity in America for granted.

The second example, this time of outsider rhetoric, comes from the memoirs of John Humphrey Noyes. Noyes was descended from respectable New Englanders; by virtue of his breeding and education, he knew a good bit about the traditions to which Campbell referred. Still, the antinomian implications in Noyes’s teachings about Christian perfectionism had led to the severance of his ties to the church to which he was first called. In remembering, long after the fact, the moment when he had lost his license to preach, he wrote, “I had now lost my free standing in the Free Church, in the ministry, and in the College [Yale]. My good name in the great world was gone. My friends were fast falling away. I was beginning to be indeed an outcast. Yet I rejoiced and leaped for joy. Sincerely I declared that I was glad when I got rid of my reputation.”¹³ Noyes called himself an outcast, and, by his other remarks, he invited historians to start plugging him into a narrative role either as victim, as disrupter, or as heroic protester. But his statement, if pressed too quickly into the service of any one of these narrative lines, can be misleading. Both Noyes’s declaration of his outsiderhood and the opposite sort of declaration made by Campbell raise rather than settle a problem about locating American mainstreams.

As soon as we start to think about the occasion for each of these statements, the underlying reasons for such assertions, we begin to recognize almost mirror images of the explicit identifications. Campbell wrote what he did because he had grown unsure of his majority. He knew that significant groups were making claims that undercut his own claims to elite status. The self-proclaimed insider in this case defined a “mainstream” at the moment when the objective reality of that mainstream had in his own mind become problematic. A full interpretation of his statement requires our awareness of *his* awareness of cultural fragmentation. In 1867 substantial doubts existed as to whether a homogeneous Protestant mentality enjoyed much dominance, however that dominance is defined, in a country where most people did not belong to or attend a Protestant church and where the largest and fastest-growing denomination was Roman Catholic. Campbell’s statement grew from rejection as much as from self-confidence. He harbored fears about the diminished influence of men like himself.

In contrast, Noyes’s explicit statement of rejection exuded self-confidence rather than fear, a sense of growing influence rather than a feeling of social isolation. Clearly, when Noyes emphasized the hostility directed at him, his alienation from his neighbors and their values, he was not proclaiming his insignificance or irrelevance. His words, contained in a journal written for future generations, were rather

¹² Campbell, “Christianity and Civil Liberty,” *American and Presbyterian Theological Review*, 5 (1867): 390–91.

¹³ Noyes, “Confession of Religious Experience,” as compiled in George Wallingford Noyes, ed., *Religious Experiences of John Humphrey Noyes, Founder of the Oneida Community* (New York, 1923), 125.

chosen for the effect they had in stressing his importance. His enthusiastic admission that he was an outcast proceeded from a deeply held belief that he had achieved, whatever the paradox, a status and respectability that both God and his enemies took seriously. He tied his rhetoric into the ancient tradition of religious sacrifice that a long line of Christian saints and martyrs had made sacred. He could be certain that his American readers would not miss the point.

Spotting these counterimages does not require any between-the-lines reading. Indeed, they are explicitly stated in other passages drawn from Campbell's and Noyes's works. Persecuted individuals like Noyes, who in more recent times have typically wrapped themselves in the flag, have frequently dropped the rhetoric of deviance to emphasize their embodiment of the "true" American spirit. Reverend Campbell, along with virtually all other Protestant ministers in the nineteenth century, could scarcely utter a paragraph proclaiming the dominance of Protestantism in America without adding another bemoaning its decline. From early in the century, they regularly trotted out the Jeremiad sermon to attack "secular" editors and politicians who gave them no help in their efforts to curb the Catholic menace, combat infidelity, or enforce Christian moral standards. H. A. Boardman, the pastor of the Walnut Street Presbyterian Church in Philadelphia, stated flatly in 1840 that the "secular press" was "decidedly un-protestant, if not anti-protestant," and that "public sympathy" was with Catholics. "It is a remarkable fact," he said, that, if any one of the Protestant sects "rises up to withstand the bold and threatening aggressions of Romanism, it is sure to draw down upon itself the imputation of a self-aggrandizing and persecuting spirit."¹⁴ The complaint had many echoes. People who saw themselves in one mood as guardians of dominant American values seized regularly on the language of the martyr. Insider and outsider identities are rarely unmixed. In fact, when the identifications are of most interest to historians, they are interlocked. The assertion that one is an outsider often implies the opposite, and vice versa.

This point can easily be pushed too far. Obviously, the rhetoric of self-proclaimed insiders and self-proclaimed outsiders tells us a great deal about the social and economic realities that prevailed in a given period of time. Whatever Campbell did or did not say, America was in some important factual respects a Protestant nation in the mid-nineteenth century. Protestants, even if that term only signified any non-Catholic who adhered to a vague sort of Christianity, could rely on the support of many institutions (the public schools, for example). Compared to Irish-Catholic immigrants, native-born Anglo-Americans had unproblematic identities, and they held the vast majority of key economic and political positions. And, whatever Noyes did or did not say, he confronted throughout his career customs and laws that were directly antagonistic to his goals. The burdens of outsiderhood, no matter what boost they give to a sense of self-importance, are quite real. The social costs that Noyes paid for his outsider identity were, relatively speaking, light, which no doubt explains the unusual cheerfulness in his remembrances of the circumstances that made him an outcast.

¹⁴ Boardman, "Is There Any Ground to Apprehend the Extensive and Dangerous Prevalence of Romanism in the United States?" in *Anti-Catholicism in America, 1841-1851: Three Sermons* (New York, 1977), 31, 33, 35.

Although there most likely was some sort of connection between rhetoric and factual reality, there are particular reasons why these connections often became tenuous in conflicts between American insiders and American outsiders. The historian who quotes Campbell to prove that dominant American culture was purely and simply Protestant or who quotes Noyes to prove that “complex marriage” offended the moral standards of most Americans is asking for trouble. Mistakes start piling up once the historian forgets about the wide gap that is maintained in history between a factual reality, which keeps the same face, and a perceived reality, which does not. The assertions that historical actors made to identify insiders and outsiders have to be considered in part as metaphors and as strategic fictions. Such statements do not—cannot—tell us all there is to know about power and status in American life, for they were often the ways in which power and status were contested. A group that calls itself the moral majority or a political party that titles its journal *Mainstream* is trying to shape perception in a particular way.¹⁵ The label does not necessarily describe the group’s numerical strength. Indeed, such labels usually aim at trying to change the facts by taking what strategic advantage there is in being identified with the majority. The reasons behind this strategy may seem obvious—at least to anyone who has read Tocqueville on America. What American historians more often miss, however, in relating their stories about insiders and outsiders are the advantages that can be drawn from the identification as an outsider—advantages that significantly alter the roles, and nonroles, that historians have usually given outsiders in historical narrative.

RHETORIC FROM THE PAST GENERALLY GIVES HISTORIANS no choice but to treat some groups as insiders and others as outsiders. At the same time, historians cannot forget about the counterimages running through the rhetoric or about the impressive degree of popular and elite support that the values of outsiders often enjoyed. The question now becomes, What can happen if historians do?

In this section, I shall ignore differences between particular schools of historical narrative (but return to those in the conclusion) and concentrate on how stereotyped treatments of outsiders can cause blind spots in historical judgment. I know that common narrative stereotypes about outsiders capture some aspects of historical reality well enough. I am concerned with those instances in which the stereotypes mislead us about the meaning and uses of outsiderhood in American culture, for trying to characterize outsiderhood simply in terms of objectively measured distances from an objectively located, unshifting mainstream is impossible. No matter what rhetoric may seem to say, sometimes to be an outsider can really mean, both objectively and figuratively, to stand at the center of American experience.

The notion that historical outsiders stand at some measurable distance from the dominant (typical?) concerns of American life is perhaps most strongly reinforced

¹⁵ One journal of American communism, published between 1956 and 1963, bore the title *Mainstream*; before 1956, the name of the journal was *Masses and Mainstream*.

in our conceptualization of the outsider as victim. We analyze victims as people who consciously or unconsciously violate social norms and are punished as a result. We can cite almost endlessly the words of historical outsiders who recall for us the ridicule of their friends and neighbors when they dared to be different. According to Joseph Smith, his relating the story of his controversial vision of 1820 “excited a great deal of prejudice against me . . . and was the cause of great persecution, which continued to increase.”¹⁶ The American historical record is filled with people who suffered savage discrimination because of clear differences between themselves and their surrounding culture. The sorriest tales relate to non-English-speaking immigrants, to native Americans, and to black slaves.

Clear and obvious differences do not, however, tell the whole story about many historical outsiders. The indisputable facts about the harassment and brutal murder of Joseph Smith make Smith an outsider in any historical narrative. Whether he remembered the stories about his early persecution with unfailing accuracy or not, his translation of the Book of Mormon and his founding of the Church of Jesus Christ of Latter-day Saints clearly caused him trouble in his upstate New York community. Those Americans who joined the Mormon Church crossed sharp boundaries drawn between themselves and surrounding “gentile” settlements and were eventually driven into exile. Yet historians know some equally important things about Joseph Smith and his followers. Those persecuted outsiders were clearly optimistic, pragmatic, and hardworking—a group of people whose religion “brought together various impulses and ideas of the emerging American *Weltanschauung*.”¹⁷ If they were so like many other Americans, why did the practices of their religion, which did not for a long time include polygamy, cause such severe social consequences?

That question cannot be answered without taking into account the complexities of outsider identification and the ways in which Smith controlled his role as an outcast. That is to say, the deep divisions that sprang up between Mormons and many other Americans cannot be explained entirely by reference to differences between Smith’s beliefs and those championed by powerful Protestant institutions. Labeling theories of deviance are surely useful for understanding the nineteenth-century Mormon controversy.¹⁸ Outsiders (deviants) do get more or less arbitrarily stigmatized whenever the legally dominant part of the social order wants to draw boundaries. The theory explains why the persecuted outsiders in fact often share the basic value structure of the persecuting insiders. Groups whose objective deviance from existing norms is vast can safely be ignored by the dominant parts of society. What is not sufficiently emphasized in labeling theory, however, is the degree to which both parties to a controversy have active parts in drawing

¹⁶ Smith, *History of the Church of Jesus Christ of Latter-day Saints*, as quoted in Fawn M. Brodie, *No Man Knows My History: The Life of Joseph Smith, the Mormon Prophet* (2nd edn., New York, 1971), 23.

¹⁷ A. Leland Jamison, “Religions on the Christian Perimeter,” in James Ward Smith and Jamison, eds., *The Shaping of American Religion*, 1 (Princeton, 1961): 214–15. Tolstoy, in an oft-quoted conversation with Andrew Dickson White, said that the Mormons taught the American religion.

¹⁸ See Kai Erikson, *Wayward Puritans: A Study in the Sociology of Deviance* (New York, 1966); Robert A. Dentler and Kai Erikson, “The Functions of Deviance in Groups,” *Social Problems*, 7 (1959–60): 98–107; and Howard Becker, *Outsiders: Studies in the Sociology of Deviance* (New York, 1963).

boundaries. Smith may innocently have crossed some lines that others suddenly made an important test of social conformity. But Smith did a great deal subsequently to keep those lines important. By the time he wrote "the Lord has constituted me so curiously that I glory in persecution," Smith had learned to exploit his identification as an outcast.¹⁹ Like John Humphrey Noyes, he responded to persecution by letting his imagination elaborate and dwell upon what it meant to be a victim.

One of Mormonism's frustrated nineteenth-century critics complained that the "Mormons always have, and ever will thrive by persecution. They know well the effects it has upon them, and consequently crave to be persecuted."²⁰ We need not accept "crave" as the proper verb to recognize that Mormon history does seem to bear out the observation of Georg Simmel that "groups, and especially minorities, that exist in struggle and persecution, frequently rebuff approaches and tolerance from the other side, because otherwise the solidity of their opposition would disappear, and without this they could not further struggle."²¹ More recently, Luther Gerlach and Virginia Hine have noted that "oppositional" movements often perceive more opposition to their cause than seems to exist objectively. That "unrealistic" notion of opposition results not from paranoia but from the functional benefits it can obtain for the movement's strength.²² Persecution, real or imagined, can breed success.

Joseph Smith and other nineteenth-century Mormon leaders did not discover the principles of contemporary sociology and make them central to their plans; they did not in high and secret councils choose strategies that deliberately invited misunderstanding and persecution. They did, however, choose strategies that insisted on their distinctiveness. Knowing that they had considerable control over the way that other people perceived them, they reacted to the widely advertised belief that they were different by seeking to reinforce it. Their public speeches and writings made no attempt to reconcile the differences they had with the gentiles. They sought to maximize, rather than minimize, the importance of the innovative features of their church. Mormons, not gentiles, authored the notion of a separate Mormon culture. It was not distance from a mainstream that made that idea credible; it was, rather, the consequence of a conflict in which all parties found strategic reasons to stress not what Mormons had in common with other Americans, which was a great deal, but what they did not have in common. The mutual exaggeration of differences, which encouraged the idea that the differences could not be peacefully resolved, gives historians a considerable problem in trying to relate this controversy between insiders and outsiders to questions they have about dominant and subordinate value systems in American life.

¹⁹ Smith, "Address of May 26, 1844," in his *History of the Church of Jesus Christ of Latter-day Saints*, as quoted in Donna Hill, *Joseph Smith: The First Mormon* (Garden City, N.Y., 1977), 392.

²⁰ Dr. Garland Hurt to George Manypenny, May 2, 1855, as quoted in Norman F. Furniss, *The Mormon Conflict, 1850-59* (New Haven, 1960), 51.

²¹ Simmel, "The Sociology of Conflict," *American Journal of Sociology*, 9 (1903-04): 680.

²² Gerlach and Hine, *People, Power, and Change: Movements of Social Transformation* (Indianapolis, 1970), chap. 11, esp. pp. 184-85. For the best discussion of the means used to build group commitment, see Rosabeth Kanter, *Commitment and Community: Communes and Utopias in Sociological Perspective* (Cambridge, Mass., 1972), esp. pt. 2.

A similar sort of problem is raised by the struggles of Roman Catholics, another important religious group that is usually given a victim's role in historical narratives about nineteenth-century America. These narratives, which strongly condemn nativist attacks upon Catholicism, end with the happy conclusion that the Church prospered in adversity. Indeed it did; but, as with the case of Mormonism, that result was not entirely fortuitous, nor did the persecution of the Church result solely from the affront it raised to Protestant "mainstream" norms.

Orestes Brownson, a native-born American who converted to Roman Catholicism, did not believe that opposition to the Church was inevitable. If Catholics would "quietly take their position as free and equal American citizens . . . , they will gain that weight and influence in the country to which their merit entitles them. All depends on ourselves."²³ Brownson was surely much too optimistic in this estimate, just as his famous antagonist, Bishop John Hughes of New York City, charged. But the Irish-born Hughes, whose influence on the course of American Catholicism was far greater than Brownson's, never gave Brownson's advice much of a chance. Rather than urging Catholics to take a quiet place, Hughes learned early in his career that the tradition of outsiderhood had important uses. During the bitterly fought New York City school controversy of the mid-nineteenth century, Hughes played a central role in destroying the Public School Society and replacing it with a school administration less obviously hostile to Catholics. Although Hughes was as much a victor as anyone in the dispute, public statements stressed the opposition that his positions aroused rather than the support they received. Undeniably a powerful man, he rarely lost an opportunity to review the "vituperation, calumny, and slander" heaped on him.²⁴

"Americanizers" in the Catholic Church were always critical of Hughes's militance and of what they regarded as his choice to separate Irish-born Catholic immigrants from the larger currents of American nationality. In 1967 Andrew Greeley wrote that Hughes "did not like American society. . . . He could see nothing but hostility and persecution." Another Catholic scholar, David O'Brien, has argued that Hughes encouraged a "ghetto mentality" among American Catholics, that he did little to assist "his immigrant followers to understand their surroundings and to live in peace with their neighbors."²⁵ Both judgments are in part correct, although they seriously underestimate the kinds of strength and influence Hughes managed to conceal under the label "victim." Hughes in fact liked America and often said so. Denying that America was *de jure* a Protestant country, he believed that the American Catholic Church had enjoyed great success. Nonetheless, he concentrated on the paradoxical strategy of pursuing the power of the "insider" by keeping the stigma of outsiderhood constantly attached to the people he tried to lead. Hughes and most other nineteenth-century American Catholic leaders developed a vested interest in supporting the claims of nativists that America was *de facto*

²³ Brownson, "Mission of America," *Brownson's Quarterly Review*, New York ser., 1 (1856): 414.

²⁴ Hughes, Open Letter to Mayor James Harper of New York, May 17, 1844, in his *Complete Works of the Most Reverend John Hughes, D.D.*, 1 (New York, 1866): 454.

²⁵ Greeley, *The Catholic Experience: An Interpretation of the History of American Catholicism* (Garden City, N.Y., 1967), 107, 117, 125; and O'Brien, "American Catholicism and the Diaspora," *Cross Currents*, 16 (1966): 310–15.

overwhelmingly a Protestant country. By and large they left it to the outspoken “Americanizers” among them to dream idly of a Catholic America while they forged a more important myth that posited almost a natural connection between Catholic outsiderhood and Catholic power. They perpetuated vague notions about an American mainstream of Protestant values in order to make victimization a usable tradition.

Interpretive difficulties similar to those that are created in narrative versions of the outsider as victim appear in narratives that stereotype outsiders as agents of social change. The assumption that outsiders stand at a measurable distance from a set of dominant values encourages the conception of outsiderhood as something that inevitably promotes social change or disruption. Historical figures who did not share what are posited to be mainstream concerns must, it is argued, pose a threat to those concerns. Their discontent, whether it serves progressive or regressive ideologies, acts in narrative as a challenge to the status quo. The historian’s own biases may make the pattern a useful form of narrative. But, as is true of our images of historical victims, those biases find much support in the statements of historical figures. Often the same historical statements that stress victimization tell of the holy causes for which victims struggled.

Perhaps we do not need reminding that the causes of historical outsiders were not always very holy. The persecutors (not merely the persecuted), the Know-Nothings (not merely the Catholics), the mobs that attacked the abolitionists (not merely the Garrisonians)—all at one time or another posed as beleaguered outgroups. But their posited victimization may suggest another oft-overlooked point: outsider rhetoric, regardless of the group or individual employing it, has some uses that are as likely to thwart change as to promote it. The quickest way to make this argument, although the main point has nothing to do with a particular political persuasion, is simply to note how often American conservatives of one kind or another have resorted to outsider rhetoric. The case of Henry Adams should remind us that people who possessed elite social status by any reasonable view of the facts did not necessarily perceive of themselves as part of the American “mainstream.” If Adams seems to represent an extreme case, an eccentric attracted to a kind of conservatism that had never been comfortable with American institutions, then consider a recent representative of the more typical school of American free-enterprise conservatism. William Simon, a man of considerable wealth and a recent secretary of the treasury, suggested in memoirs published in 1978 that he was an outsider. “The United States,” he wrote with a passion verging on bitterness, “is now ruled, almost exclusively, by a political-social-intellectual elite that is committed to the belief that the government can control our complex marketplace by fiat better than the people can by individual choice.”²⁶

It would be a mistake, I think, to write Simon’s comment off as mere hypocrisy. In America’s past, outsiders have taken their stand at the top and the bottom and the middle of the social scale. And, whatever our perception of the politics of any of them, they have all in some sense invented a mainstream against which to direct

²⁶ Simon, *A Time for Truth* (New York, 1978), 41.

opposition. Obviously, some inventions are more “real” than others, and some oppositions more serious and less chosen than others. We should at least recognize, however, that a common strategy of exaggeration can be found in the writings of Eugene Debs as well as in those of Simon and that the exaggeration, rather than creating a verbal space for radicals to sustain their dreams, just as often created a verbal space for conservatives to vent their despair.

Every declaration of outsiderhood, even Simon’s, is on one level made with the intention of changing something. But the rhetoric, whenever it expresses a well-developed form of self-identification, also means to conserve, in a nonpolitical sense. Victor Turner has shown in what ways a “liminal” or “underground” identity can act as a challenge to the existing social structure.²⁷ Yet, even the pose of the revolutionary can quickly become domesticated, not because the “system” somehow finds a way to crush revolutionary zeal but because the identity becomes important for itself. An outsider identification pursued by a group over time can provide the group with well-recognized social status within the structure of existing social arrangements. Mormon self-identity was initially shaped by the radical notion that those who followed Joseph Smith were preparing the way for the transformation of America into the Kingdom of God. But the longer Mormons waited for that kingdom, the more their outsider identification took on a life of its own. It concerned itself less with changing America than with preserving the social structures that made the identification significant. The Mormon sense of uniqueness was maintained by preserving—conserving—an oppositional relation to a perceived mainstream. At the same time, the effective desire to alter that mainstream lessened and to preserve—conserve—the status quo increased.

Andrew Hacker has noted that ethnicity, one form of American outsider identity, may be a form of consciousness that keeps people from making trouble for the system. The cultivation of ethnicity allows group members to make the best of their relatively low social standing by finding psychic satisfaction in their limited opportunities.²⁸ Much the same point could be made about the kind of consciousness sponsored by many nonfeminist women’s groups in America’s past and present. The conservative uses of outsiderhood must almost always qualify the use of the category in historical narrative to emphasize a potential force for change or disruption. An opposite potential also exists, even if it is rarely directly expressed in outsider rhetoric. Historical narrative may properly champion the cause of economic underdogs. But that commitment should not erase our awareness that outsiders often had a substantial investment in many of the same cultural and social values that maintained the status of insiders. The image of outsiders as agents of change, no more than the image of them as victims, does not capture their full importance for American historical narrative. Certainly, a priori judgments cannot tell us whether outsiders were more likely than insiders to promote the cause of social justice.

²⁷ Especially see the essays in his *Drama, Fields, and Metaphors: Symbolic Action in Human Society* (Ithaca, N.Y., 1974) and *The Ritual Process: Structure and Anti-Structure* (Chicago, 1969).

²⁸ Hacker, Review of Stephan Thernstrom, ed., *Harvard Encyclopedia of American Ethnic Groups* (Cambridge, Mass., 1980), in the *New York Times Book Review*, February 1, 1981, p. 29.

This point raises a third common problem in the narrative treatment of outsiders. Noticing outsiders only as agents of change often leads to almost total neglect of important outsider groups, a neglect that is yet another option that a great deal of historical rhetoric invites. Historical outsiders frequently used words to suggest what we now call marginal social status. The exaggeration of their differences from others cannot help but carry that implication. Whether historians decide to pick up the invitation should depend on what they conclude about the strategies involved in outsiders' declarations that "we are not like anyone else." The decision often depends, however, on other things, such as a historian's beliefs about progressive politics; and historians as a result fail to attend to values that, while not dominant among elites, were nonetheless quite typical in popular culture and enjoyed a degree of support among elites, regardless of the explicit statements in their public rhetoric. To illustrate what kinds of confusion strategic assertions about marginality may cause, not only to historians but even to the historical actors involved, there is no better case than the experiences of Christian Fundamentalists in the twentieth century. As before, the confusion occurs when counterimages contained in the rhetoric of deviance are ignored.

Christians who coupled insistence on a literal interpretation of Scripture with premillennialism began to find themselves written out of the Protestant mainstream in the late nineteenth century. In fact, ministers who favored a premillennial view of Christ's coming helped do the writing out, inasmuch as they had come to believe that they had scriptural reasons to accept the depiction of themselves as outsiders. The prophecies of the Book of Revelation suggested that the number of true Christians would dwindle to a small, suffering minority as the last days approached. The "small remnant" mentality grew stronger among premillennialists during the first part of the twentieth century.²⁹ National press reaction to the Scopes-Darwin trial in the 1920s was only one of the causes. By the end of the 1920s, leading Fundamentalist ministers were declaring that their efforts to prevent Christian Liberals from gaining a dominant position in the largest Protestant denominations had failed. This was by no means the case, but the perception was important in what happened next. With some considerable success in the 1930s and 1940s, the most uncompromising of the Fundamentalists tried to organize the faithful into separate churches and associations. Although they put a much different interpretation on their announced "failure" than Liberal Protestants did, they outdid everyone in attesting to the declining significance of Fundamentalism as a force in the "mainline" churches. That is to say, Fundamentalists joined with the Liberals, who by and large played the insiders in this particular struggle, in distorting the facts about the strength of Fundamentalist Christianity. Exaggeration, once again, was essential to outsider rhetoric.³⁰

²⁹ Pessimism was not the dominant tone in late-nineteenth-century premillennial journals and sermons. Dwight L. Moody, the best known of the premillennial ministers, was invariably upbeat. In contrast, James Brookes, in his journal *The Truth: or, Testimony for Christ*, stressed the decline of Protestant Christianity. He persisted in his work, he wrote, as "a help to the scattered witnesses for God's insulted truth, although it is certain that the protest will be unheeded, and that the voices of the witnesses, here and there, will be silenced amid the clamor of infidelity which will close the present age"; Brookes, Editorial, *The Truth*, 11 (1885): 530.

³⁰ The separatist-minded Carl McIntire was the shrillest voice in asserting Fundamentalism's rout in the major Protestant denominations. The more moderate voice of William Bell Riley had, however, begun to stress

Many American historians in writing about the twentieth century have wanted to emphasize a theme of secularization. For that reason, in analyzing the consequences of the Scopes-Darwin trial in Dayton, Tennessee, they were more than happy to take the statements of Fundamentalists as evidence that their movement became something of a fringe cult after the 1920s, a refuge for the alienated and the dispossessed. The anti-Darwinism and the anti-intellectualism that historians correctly understood as basic to the Fundamentalist stand were not things they saw as helpful to a rational political order. Given all of the other troubles that stood in the way of the political enlightenment of Americans, historians wanted to write Americans beyond their religious backwardness as quickly as possible.

Unhappily for the proponents of secularization theory, various forms of conservative Christianity, which included Fundamentalists but also included groups more specifically labeled neo-evangelical and pentecostal, staged a dramatic comeback in the 1970s.³¹ That in any case was what the media reported. The wealth of publicity that attended this “resurgence” put Fundamentalists and historians alike in something of a quandary. Fundamentalists faced an identity crisis. They had to settle what George Marsden has characterized as their “strikingly paradoxical tendency to identify sometimes with the ‘establishment’ and sometimes with the ‘outsiders.’”³² They had to worry about the cost of accepting an image—being tailored for them in the press—that settled the paradox by placing them firmly in the “mainstream” of Protestant Christianity. Recent issues of the glossy journals of conservative Christianity, themselves a sign of what can superficially be transformed as a result of media attention, have to an unusual degree been filled with hand-wringing discussions about the dangers of “respectability.”³³ And with good reason. The outsider’s stance, which in this case was frequently accompanied by a strident patriotism, had worked rather well for the Fundamentalists, keeping the faithful militant even in those churches where the movement had supposedly died. The wholesale switch to an insider’s identity contained risks; among other things, it voided the assumption that until Christ’s coming the hypocrites must control the churches. A peace treaty with Liberals clearly eliminates one primary reason why

the defeat of Fundamentalism by the end of the 1920s. Louis Gasper, in describing the mood of Fundamentalists in the 1930s, wrote, “For the most part they acted like martyrs and regarded themselves as the ‘faithful remnant’ of modern times suffering for Jesus Christ. This was a psychological stimulant, because they believed their faithfulness would be rewarded posthumously.” Gasper, *The Fundamentalist Movement* (The Hague, 1963), 21.

³¹ For an influential book that guided much of what was subsequently said, see Dean M. Kelley, *Why Conservative Churches Are Growing* (New York, 1972). Also see Richard Quebedeaux, *The Young Evangelicals: Revolution in Orthodoxy* (New York, 1974), and *The New Charismatics: The Origins, Development, and Significance of Neo-Pentecostalism* (Garden City, N.Y., 1976).

³² Marsden, *Fundamentalism and American Culture: The Shaping of Twentieth-Century Evangelicalism, 1870–1925* (New York, 1980), 6.

³³ See, for example, David F. Wells, “The Reformation: Will History Repeat Itself?” *Christianity Today*, October 25, 1974, p. 8. In the same journal, Timothy L. Smith remarked on the “fortress mentality,” which he saw as still dominant among evangelicals; see “A Fortress Mentality: Shackling the Spirit’s Power—An Interview with Timothy L. Smith,” *ibid.*, November 19, 1976, pp. 23–24. Also suggestive are David Moberg, “Fundamentalists and Evangelicals in Society,” in David F. Wells and John D. Woodbridge, eds., *The Evangelicals: What They Believe, Who They Are, Where They Are Changing* (Nashville, Tenn., 1975), 143–69; and Martin Marty, “Tensions within Contemporary Evangelicalism: A Critical Appraisal,” *ibid.*, 170–88.

Fundamentalists had been influential. All of their previous rhetoric had implied that the acceptance of majority or mainstream status meant defeat.

The quandary of historians is of a different sort. Realizing that things rarely happen as suddenly as journalists like to think, historians must come to grips with the likelihood that the relative number of liberals and conservatives among Protestant believers has changed less dramatically over the last sixty years than historical narrative has suggested. Of course we have no reliable way to be absolutely sure. We can tally up leaders and militants in each camp, but those figures do not get us very far in estimating what sorts of belief attracted less active church members. Nonetheless, a decent regard for historical continuity suggests that, although recent media accounts have swayed the way liberals and conservatives have perceived their relative strength in the Protestant camp (not unimportant in itself), they have overstated the degree to which belief actually changed.³⁴ Therefore, judgments that have made Fundamentalism a movement of drastically dwindling significance in the twentieth century, although they have reflected fairly enough one image in the historical rhetoric, have ended by distorting historical reality in the worst possible way. Historians, by ignoring the contradictory elements in self-perception, the counterimages in Fundamentalist rhetoric, have caused a good chunk of the American population to disappear. Such benign neglect can serve admirable moral purposes in narrative. Yet it produces history that many Americans cannot recognize as their own.

THE LINES BETWEEN HISTORICAL INSIDERS AND OUTSIDERS may seem so hopelessly muddled in subjective perceptions that American historians ought simply to drop any suggestion of those categories in constructing narrative. We are stuck with some fundamental problems, and one of my purposes here has been to clarify the reasons why we can never definitively resolve such questions as when or even whether Roman Catholics entered *the* American mainstream. Even if we accept the common opinion that they did enter the mainstream sometime in the twentieth century, we cannot prove the case merely by referring to such undeniably important information as rising intermarriage among Catholic ethnic groups, the upward movement of average Catholic family income, or the election of John F. Kennedy as president. With respect to purported mainstream boundaries, these facts are important only to the degree that they prompted American Catholic and Protestant leaders to modify dramatically their nineteenth-century rhetorical patterns.³⁵ Making sense of those patterns is a more difficult chore than citing demographic information or election results. The same caveat applies to the study of other groups. The opinion of one historian that Utah's Senator Reed Smoot led a twentieth-century Mormon march into the American mainstream has to be viewed

³⁴ For an estimate of the continuity of Fundamentalist strength, see Joel A. Carpenter, "A Shelter in the Time of Storm: Fundamentalist Institutions and the Rise of Evangelical Protestantism, 1929–1942," *Church History*, 49 (1980): 62–75.

³⁵ For a useful collection that offers varying perspectives on the Church's place in America, see Thomas T. McAvoy, ed., *Roman Catholicism and the American Way of Life* (Notre Dame, Ind., 1960).

cautiously. Present-day Mormons, despite all of the chatter about their having become "super-Americans," still know how to keep their rhetorical distance from the mainstream when it suits their purposes.³⁶ And, to complicate the matter further, Mormons' use of a less strident language of differentiation does not in itself prove that the Mormon world view has undergone a transformation that in some easily defined, objective way has "normalized" their system of belief.

Given the almost endless variety of possible misinterpretation, the temptation to abandon the effort to make sense of the complex relationship among particular events, perception, and rhetoric is strong. But this relationship, with all of its inherent and attendant problems for historians, is the very thing that gives historical importance to questions about insiders and outsiders. We should not, therefore, reduce the narrative uses of insider and outsider categories but, instead, allow our narratives the chance to heighten rather than conceal the ambiguities. Perhaps the time has come for American historians to stop using the particular word "mainstream." It carries too many normative connotations about what values most Americans really did accept. Its casual and habitual use invariably deflects thought away from difficult problems about how to discuss typical or dominant values. An overused metaphor, it works to dismiss the fictional aspects of insider and outsider labeling and invites simplified conceptual versions of what is majority and minority culture in America.³⁷ Carried through, however, with a proper appreciation for the reasons why distortion and exaggeration are part of the meaning and why outsiders on one issue may be insiders on other issues, a narrative focus on contests between insiders and outsiders remains one of the best ways to analyze America's past. It works because the contests themselves were typically American, rather than necessarily the values that one side or the other proclaimed. Historical analysis of the contests is a means to open up, not settle prematurely, a multitude of questions about what were typical or dominant values in America's past. Such analysis must examine the social costs and benefits that attached to insider and outsider identifications on a case by case basis.

It is, of course, one thing to argue that the most common patterns of historical narrative do not fully reveal the complexity of these issues and quite another to

³⁶ For various issues relating to the "Americanness" of the Mormons, see Klaus Hansen, *Mormonism and the American Experience* (Chicago, 1981), esp. chaps. 2, 8; the essays in Marvin Hill and James Allen, eds., *Mormonism and American Culture* (New York, 1972); John Sorenson, "Mormon World View and American Culture," *Dialogue: A Journal of Modern Thought*, 8 (1973): 17–29; and Editorial Introduction: "In This [Fall] Issue," *Utah Historical Quarterly*, 45 (1977): 323. In a January 1979 syndicated column, the conservative columnist George F. Will wrote, "Mormons comprise the most singular great church to come into existence in the United States, and it is quintessentially American."

³⁷ In addition to the national, sociopolitical mainstream, there is apparently a mainstream tendency in every subcategory of American life. Recent articles in the *New York Times* have announced that Hispanics are "beginning to have a broad impact on the economic, social, and cultural life of mainstream America," that "courses on ethics . . . have moved into the mainstream of American universities and professional schools," that conservatism, once a "backwater of American intellectual and political life, has spilled into the mainstream," and the Village People "sociologically . . . attest to the continuing permeation of homosexual ideas into the mainstream." References pulled more or less randomly from scholarly works about America posit such things as "mainstream political discourse," "a Marxian mainstream," "mainstream feminism," and "mainstream Pentecostalism." With respect to normative overtones of the metaphor, remember that "mainstreaming," as an educational goal, is a "good thing"; it is the process by which the aspirations of "normal" men and women become realizable by everyone.

posit an alternative. Any usable suggestions must derive from recognizing what is right and wrong in the typecasting of outsiders found in the major schools of American historiography. Actually, a good bit of what I have said here bears out some important themes of Consensus interpretation. This essay has, after all, pointed out that differences between ostensibly antagonistic groups have often been more apparent than real. It has, furthermore, supported the notion that centers of meaningful power really have been dispersed in American life. The rituals of outsiderhood, whatever they might seem to imply about alienation or victimization, have often been the means of finding or even maintaining a well-established, comfortable place in American life. The tradition of dissent, and the moral authority and respect dissent can command, have contributed to the relative stability of a plural system that even its architects had feared might quickly collapse.

I do think that many of the insights of Consensus historians are essentially correct. We know that at least some immigrant groups into this country, even in the late nineteenth century, shared more with the host country than was generally recognized.³⁸ We know, too, that many immigrant groups, having gone through a cultural assimilation, continued to resist what Milton Gordon called “structural assimilation.”³⁹ We know that many “ethnics” stressed their differences from other groups in America, even when their sense of those groups remained vague, and cultivated in public rhetoric the feeling that “this country did not belong to them.” This last sensibility continues to inform the strategies of the so-called new ethnicity. Michael Novak’s *Rise of the Unmeltable Ethnics*, published a decade ago, fits into the well-established tradition of making outsiderhood count to one’s advantage.⁴⁰ Novak’s re-invention of the Protestant mainstream and his exaggeration of the differences between Catholics and Protestants were his ways of cutting WASP America down to size. People like Novak insisted, of course, that the American mainstream was shifting to include ethnic groups—that “America really was becoming America.” But, if America ever did become America in Novak’s sense, then our variegated national culture would be indistinguishable from a homogeneous one. Differences remain important only if someone asserts their existence and takes them seriously.

As it happens, a failure to appreciate this last point has seriously disabled Consensus interpretation. If Consensus historians have understood well enough that differences between groups have often been exaggerated, they have not been very sensitive to the reasons why many asserted, and even imagined, differences have so effectively divided people. A Consensus viewpoint might correctly spot “phony” aspects of Novak’s ethnicity as judged by some objective standards, but it

³⁸ John W. Briggs, *An Italian Passage: Immigrants to Three American Cities, 1890–1930* (New Haven, 1978), 272–78.

³⁹ Gordon, *Assimilation in American Life: The Role of Race, Religion, and National Origins* (New York, 1964), esp. chaps. 2, 3, 6.

⁴⁰ Novak, *The Rise of the Unmeltable Ethnics* (New York, 1972), esp. 56, 62, 71, 291. During World War I, at the height of the frenzy to get the melting pot boiling, Arthur Preuss wrote, “The man or woman who would tamely submit to a large part of what goes under the name of Americanization is not fit to be in America”; Preuss, “Americanizing the Immigrant,” *Fortnightly Review* [St. Louis], 25 (May 15, 1918), 156–59, as quoted in Richard M. Linkh, *American Catholicism and European Immigrants, 1900–1924* (New York, 1975), 22–23.

has trouble making sense of the ways in which “subjective” perception itself becomes “objective” historical information, to be analyzed and interpreted just like crime statistics or marriage patterns or election results. Suggesting that antagonism in American life was somehow always the result of misunderstanding, as Louis Hartz did, imposed severe handicaps on historical imagination. Consensus narrative tended to render cultural differences (“real” outsiderhood) either relatively rare—and, hence, historically unimportant—or rapidly disappearing—and, hence, of no great moment. Since Consensus interpretation could not adequately explain why Americans quarreled, it has encouraged us to think that they did not. The quarrels that did get noticed were deprived of substantive content. The argument that perception does not count dissolved differences. The possibility that competing groups turned similar values to very different uses was not explored.⁴¹ These failures contributed to what was surely the biggest mistake in Consensus interpretation: the notion that the dispersal of power in America had led to a rough equality among groups. To say that self-proclaimed outsiders often gained an effective control over their lives and that self-proclaimed insiders were often insecure (as this essay has argued) is not to demonstrate that groups, of either the insider or the outsider variety, have had an equal say in American life.

Schools of historiography that have stressed conflict, therefore, began with a solid advantage over the Consensus school. Rather than making a disappearing line between insiders and outsiders the grand theme of American history, they have recognized that American experience is unintelligible without giving central importance to the lines insiders and outsiders drew, and consciously perpetuated. The aim of narrative should not be to drain those contests of meaning, because the participants had not realized their common commitment to Locke, but to explore the sorts of social meaning, real or imagined, they created. From the standpoint of this essay, what is weak in Progressive and Radical narrative is the tendency to turn all contests into sociopolitical struggles. Insiders were important only as the normal occupants of the most powerful positions in America and as the defenders of the status quo. Outsiders were important only as the economic underdogs who, because they had no social standing to protect, championed greater social justice. This sort of meaning is only one among many possibilities. As we have seen, outsiders may have had a significant social status and selfish interests to protect. They may have victimized as well as have been victims. And they may have had an importance to American culture even if they did not criticize things that some historians think they should have.

Undeniably, the number of American outsiders being recovered in historical narrative is growing at an impressive rate. That process has inevitably called attention to some of the complexities and ambiguities discussed in this essay. Radical historians especially have demonstrated that many groups and individuals, formerly thought to be without power or significant influence, had both. Thus, Herbert Gutman, in a postscript to a justly famous article, chided an earlier

⁴¹ For one recent exploration of this possibility, see David Montgomery, *Workers' Control in America: Studies in the History of Work, Technology, and Labor Struggles* (New York, 1979), esp. Introduction, chap. 1.

generation of labor historians for accepting the view that “the Gilded Age radical lives outside the mainstream of his times.” In following the “route” of the Paterson, New Jersey, socialist leader Joseph McDonnell, Gutman found a way to “fit the Gilded Age labor radical into the mainstream of that era’s history.”⁴²

For several reasons, however, I am not completely reassured by present trends. Outsiders are still taken up in narrative largely for the political significance they can be assigned. The full importance of outsiderhood in American historical experience will continue to go unrecognized until we are willing to see that not all, perhaps not most, outsiders have been “nice” or egalitarian-minded people. Once we finish studying the many historical groups whose protests remain politically important in our own time, we may find that we still have not got very close to writing a “people’s history of the United States.” Too much will remain buried, either with pity or contempt, under the label “fringe” or “marginal.”

Second, to turn back to the question that began this essay, we do have to ask whether “mainstreaming” people who were once considered outsiders or passive victims is exactly the right narrative tactic. Constructing history so that Joseph McDonnell becomes one of the founders of the welfare state may only be Consensus history with a politically raised consciousness. It may reflect well enough some facts, yet still seriously distort the way people defined their own acts. The perceptions of historical actors, too, are facts. Inserting McDonnell narratively into an American mainstream can only undercut the oppositional strategy in his own self-identity.

Can historians ask questions about nineteenth-century spiritualists and Joseph McDonnells that can treat levels of factual and perceived reality together—questions that can assume the central cultural importance of contests between insiders and outsiders but that do not try to settle which side best reflected American norms? Clearly we can, but only if we stop assessing these contests solely for their impact on sociopolitical conflicts affecting American egalitarian principles. As a cultural process, contests between insiders and outsiders do not give the historian clear ways to divide the landscapes of the past into sub- and dominant cultures, but they do help us see that commonly held cultural values and norms did not always create common meanings or serve common purposes. To be sure, empirical research will reveal that many outsider groups had no influence, were truly deviant, and remained marginal to the major concerns of most American communities. Yet American historians cannot possibly discuss culture in America without recognizing the importance of the rhetoric and activities of opposition. (I do not therefore share the fashionable view that the study of outsiders, by showing us what America was not, can help us discern what America was.) By analyzing outsiders more carefully, we can more adequately address questions about the distribution of power and status in American life.

We cannot keep the historian’s own perceptions out of the answers, and particular ideologies will no doubt continue to inform the shape of American

⁴² Gutman, “A Brief Postscript: Class, Status, and the Gilded Age Radical—A Reconsideration,” in his *Work, Culture, and Society in Industrializing America* (New York, 1976), 262, 290.

historical narrative. Indeed, since I have argued that the perceptions of historical actors themselves not only fail as reliable guides to definitions of dominance or typicality but also confuse the interpretation of other empirical data, an ideological perspective may seem to be the only remaining way to begin a useful analysis of American social structure. I am somewhat reluctantly drawn to that point of view. But particular ideologies will, I think, lead historians in more interesting directions, if they do not merely quote insider and outsider rhetoric to support or knock down Whiggish views of American history, if they actively pursue in their narratives the ambiguities that gave the rhetoric meaning in historical time. In so doing, they will preserve the best insights of the major schools of American historiography and find better reasons than narrative now provides to look closely at groups that have not yet commanded much historical attention. After all, outsiders were as American as cherry pie not because it makes sense to award the ones we admire posthumous admission into a mainstream but because their contests with insiders were the means by which many Americans invented themselves.

Comments:

RELIGIONS GROW AND PROSPER in varying ways: some by appealing to liturgy, others by convincing theology, still others by the indisputable marks of respectability. Southern Baptists, it can now be revealed, increase in number through the instrumentality of children's choruses. Years ago, one catchy tune carried with it these words:

One door and only one,
And yet the sides are two;
Inside and outside—
On which side are you?

The door in question was, of course, that narrow entry into heaven, inside of which salvation was to be found, outside of which damnation lay. In the days of theology by chorus, "insider" and "outsider" marked the clearest of distinctions between the saved and the damned. No academic hemming and hawing allowed, the great gulf between the two groups was fixed. Now, as in many another case of popular parlance and folk understanding, things are not as simple as they used to be.

For four centuries, however, American religion has posed a plethora of terminological difficulties. Congregationalism, in seventeenth-century England a nonconformist sect, was in the Massachusetts of that time an Established Church. National churches of Sweden, Holland, Germany, and Scotland stumbled through most of America's colonial period more as vulnerable scouts than as privileged and well-armored champions. In that era, which was "sect" and which was "Church"? Classical sociological types made no sense then, nor have they been particularly helpful since, as other terms crowd onto the scene: "denomination," "cult," "schismatic," "fringe"—and more. Not only does the appropriateness of these terms vary widely from century to century, the meaning of the terms even shifts dramatically from place to place. Historians have therefore searched for more usable labels: "free" churches and hierarchical ones, charismatic and pentecostal groups as contrasted with liturgical and sacramental ones, religions of European origin versus those "made in America," or even distinctions drawn—in desperation—between the static and the ecstatic, between the historical and the hysterical.

Or the insider and the outsider. To perceive the problem is to develop some appreciation for efforts directed toward its solution. If no system of categorization "works," then how can the historian hope to treat in any detail that burgeoning, splintering, label-defying entity known as "American religion"? If no linguistic sorter separates the influential from the isolated, the typical from the aberrant, the enduring from the ephemeral, then most historians will simply turn away from the frustration of trying to examine such bewildering baggage. Is there some help through the maze?

R. Laurence Moore's argument is, of course, broader than just religion in

America, but, since the questions first confront him in this arena and since he adduces his examples therefrom, it seems appropriate to meet on that ground. How useful, then, is the language of insider and outsider in making sense out of the past? More narrowly, how useful is that language in making sense out of American religion?

When trapped in a maze, one is grateful for any way out, even if it is not the shortest cut. Professor Moore offers several clarifying directions. First, he demonstrates that “mainstream” is hopelessly overworked as a word and is generally unhelpful as an interpretive device.¹ Second, he highlights the rhetorical value of the insider-outsider tags for the participants themselves. Third, he exposes the weakness in applying these labels largely, if not solely, in a political context. Fourth, he urges historians to direct their attention to the debate itself, not to naming the opposing sides. And, fifth, he reveals that the intensity of the debate is in direct proportion to the nearness of the antagonists, or the principle of hostility in proximity. Most contests in American religion are really among the insiders struggling for privilege and position; the genuine outsider remains invisible.

IF PROFESSOR MOORE IS A GOOD GUIDE in these important ways, his analysis has been less useful at other points. For example, while trying to correct or refine our use of insider-outsider terminology, he really debases that coinage to such an extent that it can no longer circulate among historians. (It will continue to have polemical value for the involved contestants.) Nothing is wrong with getting rid of bad terms, of course, but if one consequence of so doing is that historians will be even more skittish about any venture into the thicket that is religion, then more has been lost than gained. The problem remains of finding some hard money that can circulate with trust; I will return to that question in a moment.

Professor Moore also holds up Fundamentalism as a unique instance wherein historians have misjudged importance, numbers, influence, and so forth. But American religion is filled with instances where “majorities” seem invisible or silent, while “minorities” dominate both the thinking and the writing of the historian. Many historians, for example, continued to portray the Congregational-Presbyterian “mainstream” as the dominant religious influence in the nation long after this had ceased to be the case. “Frontier” religion gained brief notice only in connection with the Westward Movement, whereas religion of the “Frontier” actually spread all across the land, East and West, urban and rural. Roman Catholicism and Judaism formed part of the immigration story in the latter half of the nineteenth century; when immigration ended, so did historical interest in these “imports.” And, because immigration was not so gripping a subject in the twentieth century, the large influx of Eastern Orthodox and Hispanics tended to go unnoticed and unstudied. Lutherans—though far more numerous than Episcopalians, Mormons, Quakers,

¹ On the former point, his tables of “essence and presence” from the *New York Times* are as devastating as they are convincing; see note 37, page 408, above. On the latter, his exposure of the contrasting meanings or images for “mainstream” among Progressive, Consensus, and Radical historians is most instructive; see pages 392–95 above.

Unitarians, and Jehovah's Witnesses, all put together—seemed neither “inside” nor “outside,” just easy to pass over. Media attention, if not historical attention, suggested that the number of Moonies or Hare Krishna chanters was only slightly below that of Baptists or Methodists. Fundamentalism has not received its due, but neither have many others.

The insider-outsider discussion, moreover, needs to be placed in the context of other, and perhaps more pervasive, ambiguities in American religion. Gallup pollsters have difficulty producing reliable data because some of the questions they ask stumble over words lacking clear referents. Persons are asked about their mystical experience, although few can offer a satisfactory definition of mysticism. How is “evangelical” understood when one is being asked to place oneself in that tradition? Beyond that, there are liberal religions and conservative religions (conservative socially? or politically? or theologically?), born-again believers (a phenomenon only of the 1970s?), and bible belt religion (restricted to the Old Confederacy?). In seeking exactitude in the historian's analysis of the past, religion in America may be the poorest place to begin.

If insider-outsider language has little utility for the historian, can we offer any other aid? Can we talk about the characteristic “Americanness” of religion in the United States? Is there some religious complex that any responsible account of the national past must take into account? Is there, in other words, a religious agenda for the historian? some religious grist? some usable component in the realm of religion? Let me suggest a few features of the landscape that must, I argue, come into the historian's view. Religious bodies that themselves have a history, either on this continent or in the culture of humankind, can receive and deserve to receive analysis within the historical guild. Historians cannot deal effectively with the spontaneous, with the fad that holds sway briefly, then is no more. Second, historians can constructively examine religion that has a body of literature, written or oral. Third, the religion that accepts some degree of responsibility for those beyond the confines of its own fellowship does, by that act, move into the historian's proper realm. Fourth, although religion can never be equated with morality, neither can it separate itself from the search for a moral center in both private and public life. This search, successful or no, cannot be a matter of indifference to the historian. Finally, the relevance of religion to the freedom of mind and of spirit, to the aspirations or illusions by which men and women live, grants to history a dimension it would not, must not, eschew.

Is this only another way of describing religion of the “mainstream”? Perhaps. I would prefer to see it as the historian's attempt to avoid not only “Balkanization” but, more importantly, the temptation to ignore the thicket that resists category and tag. For to walk warily around, or even away from, that tangled undergrowth is to distort our answer to crucial questions—Crèvecoeur's among them.

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FORTUNATELY, R. LAURENCE MOORE'S ESSAY raises more questions than it answers. I say "fortunately" with reason. There are, it seems, at least two contrasting approaches to historical scholarship. The normal approach is the "contribution-to-knowledge" inquiry—which intends to plug a gap in our existing knowledge of the past, adding to the sum of what is known. The other approach, less frequently rationalized in the professional literature but only slightly less important to a mature discipline, is what might be called an "experience-explanation" inquiry—which intends to explain some problematic historical experience. Such an inquiry may or may not add to our stockpile of historical information, but that is not its main purpose. Its purpose is rather to re-orient information, taking generally known facts and looking at them in fresh ways. For American historical scholarship, the classic "experience-explanation" inquiry is Frederick Jackson Turner's "The Significance of the Frontier in American History" (1893); other examples of the genre include Richard Hofstadter's *The American Political Tradition* (1948), Kai Erikson's *Wayward Puritans* (1966), Robert Wiebe's *The Segmented Society* (1974), and Thomas Kuhn's *The Structure of Scientific Revolutions* (1962). Professor Moore's "Insiders and Outsiders in American Historical Narrative" takes this latter approach, and, when I say it raises more questions than it answers, I mean that I find it immensely fertile and provocative.

The essay sparks the mind, leading the reader well beyond the historical and historiographical particulars that Professor Moore discussed. The author's perspective illuminates, if it does not wholly explain, some distinctly American paradoxes: why three of the last four Republican presidents—leaders of the more "conservative" of our major political parties—were all born in the Midwest but fled the American heartland for rootless southern California when they had a choice;¹ why, conversely, the last five Democratic presidents—leaders of our more "cosmopolitan" party—have all been organically rooted in a particular region of the country, and, apart from their years of public service in Washington and in the military, spent all of their lives in that region; why not since 1964 and Lyndon Johnson have Americans elected as their chief executive someone running as a Washington insider, promising responsibly to *use* the institutions of government (all other victorious candidates of late—Nixon in 1968 and 1972, Carter in 1976, and Reagan in 1980—have campaigned as political outsiders who have vowed to cut down the governmental Establishment and return it to the citizenry); why liberals seem perennially frustrated when victimized groups they championed in the past tend to grow illiberal as soon as they are let in on America's bounty; why the letter on my desk from Jerry Falwell and the Moral Majority in one breath proclaims America the greatest Force ever on earth and in the next breath announces, almost

¹ The fourth Republican president of this group—Richard Nixon—just missed being born in the Midwest, his drifter father having moved from Ohio to Orange County a few years before his birth. Nixon's own case even further indicates the southern California rootlessness of "conservative" Republican presidents.

subversively, that the American flag is about to go down the drain (and *who* is a “nervous nellie”?!); why Tom Charles Huston—author of the infamous “Huston Plan” to supplement the FBI as a domestic surveillance agency—could tell an ex-Weatherman in 1974 that the White House before Watergate felt *it* was the victimized outsider while activists on the left were being lionized by insiders of the American “Establishment”;² and finally why, on a broader scale, America on some occasions assumes an outsider role among nations of the world, believing itself providentially destined to go its own way in its own time, and on other occasions (and, sometimes, concurrently) seeks to be an insider in the world community, committing massive resources to other countries and letting those commitments dictate not only its foreign affairs but many of its internal policies as well.

SOME OF THESE PARADOXES ARE CULTURALLY INCIDENTAL, some are momentous; whatever, they suggest a wide spectrum of problems illuminated by Laurence Moore’s essay. Moore’s historical instincts are basically on target, and he has identified a major dilemma in American cultural interpretation. But the essay could be improved by better theoretical leverage on its materials and by a more precise language of explanation. This is true on several levels.

Professor Moore employs the stock label “Consensus” for American historians of the 1940s and 1950s, noting that “the general tendency of all Consensus narrative is to turn the mainstream into a broad and almost irresistible current” (page 393). That may be true of Daniel Boorstin’s *The Genius of American Politics* (1953) and Louis Hartz’s *The Liberal Tradition in America* (1955), even of substantial parts of, for example, David Potter’s *People of Plenty* (1954), John William Ward’s *Andrew Jackson: Symbol for an Age* (1955), Stanley Elkins’s *Slavery* (1959), Henry Nash Smith’s *Virgin Land* (1950), Clinton Rossiter’s *The Seedtime of the Republic* (1953). But the “Consensus” label blocks our comprehension of other rich and complex cultural visions in histories of that time—Perry Miller’s *The New England Mind: From Colony to Province* (1953), R. W. B. Lewis’s *The American Adam* (1955), Marvin Meyers’s *The Jacksonian Persuasion* (1957), Richard Hofstadter’s *The Age of Reform* (1955).

In reading Professor Moore’s disaffection from the “mainstream” metaphor, I am reminded of Lionel Trilling’s essay, “Reality in America” (1940)—his critique of Vernon Louis Parrington’s *Main Currents in American Thought* (1927–30).³ “Parrington’s characteristic weakness as a historian,” Trilling wrote, “is suggested by his title, for the culture of a nation is not truly figured in the image of the current.” “A culture is not a flow,” he countered, “nor even a confluence; the form of its existence is struggle, or at least debate—it is nothing if not a dialectic.”⁴ Trilling was right, I think, in countering the Progressivist dualism of alternating currents of Liberal and Conservative belief and in substituting a more complex, “dialectical”

² For this fascinating exchange between an ex-Nixon aide and an ex-Weatherman (each claiming to be a victim and an outsider in the late 1960s and early 1970s), see Bo Burlingham, “Paranoia in Power,” *Harper’s*, October 1974, pp. 26–37.

³ Trilling’s 1940 essay, originally published in the *Partisan Review*, is reprinted in his *The Liberal Imagination: Essays on Literature and Society* (Garden City, N.Y., 1957), 1–19.

⁴ Trilling, “Reality in America,” 7.

view of American culture. Hence, I prefer the term “counter-Progressive” for historians like Miller, Lewis, Meyers, and Hofstadter as well as for Trilling himself. The term avoids the “mainstream” cultural metaphor that elsewhere bedevils Moore; it also hints at a richer, more multi-layered apprehension of American historical experience than the “Consensus” label allows. I think such a term, with its built-in sense of the “counter,” accurately embodies what Laurence Moore is up to in “Insiders and Outsiders.” We might also think to reconsider some of the classics in the counter-Progressive form; by looking for evidence there of more than just “consensus,” we can in the rereading discover valuable insights too hastily discarded in our headlong rush over the last two decades to get “beyond consensus.”

This matter of labeling has even wider implications. For, as Lionel Trilling—through Parrington—challenged Progressives for their constricted sense of “reality” in America, Moore levels a similar complaint against categories of American historians. He suggests that the conventional labels “insider” and “outsider” point only one way, while his own reading indicates categories pointing in at least two directions, sometimes simultaneously. An expression may serve to mean just what it says (the “truth” function of ideas), or it may serve to betray concern, or doubt, about exactly the opposite of its manifest meaning (the sometimes overt, often covert, “cheer-leading” function of ideas). If a group of doctors seeks funding for cancer research, they will likely claim that medicine is an exact science and that doctors are scientific specialists; if, however, an individual doctor faces a malpractice suit for unsuccessful surgery, then he will likely switch rhetorical strategies to stress that medicine is an inexact art and that the doctor is an imperfect human being indeed. In neither instance are doctors outright liars, but they do not quite tell the whole truth either.

It takes some getting inside the particular strategy of a particular actor confronting a particular situation to know precisely what to make, and when, of the actor’s words. Professor Moore pulls this off quite well with his own historical actors—S. M. Campbell, John Humphrey Noyes, Joseph Smith, and John Hughes in the nineteenth century, Henry Adams, William Simon, and biblical Fundamentalists in the twentieth. But he does so by winging it, as it were, relying wholly on his own keen senses. I think he might do it yet better, and provide a more enduring message for his readers, if he reached past the historical profession to reinforce those senses. Moore seeks explanations that “heighten rather than conceal the ambiguities” (page 408). The field of literary criticism has been handling ambiguity for some time now, and over the years it has developed analytic tools for catching nuances of mood and temperature in ideas rather than treating them simply as block categories of belief.⁵ Lionel Trilling’s sense of an idea not as a thing nor a current but a dialectic, embodying or implying its opposite; R. W. B. Lewis’s refinement of Trilling’s notion in his vision of culture as focused in an ongoing dialogue, replete with multiple “voices” and “parties”; Bernard Bowron, Leo Marx, and Arnold Rose’s suggestion of an underlying “covert culture” running counter to the official “overt culture”—all offer historians critical tools, sharpening our senses

⁵ In this connection, especially see Lionel Trilling’s useful essay, “The Meaning of a Literary Idea,” in his *The Liberal Imagination*, 272–93.

to cultural ambiguity.⁶ Such a “literary” apprehension of reality not only might sharpen our senses but should discipline them too, making of historical ambiguity something more than mere confusion or contrariness.

LAURENCE MOORE IS ON THE VERGE OF A LANGUAGE that could offer a gestalt switch for the conceptual dilemmas besetting the profession. He writes of a “rhetoric of deviance” (page 390), “historical actors” (page 391), “insider and outsider rhetoric” (page 392), “strategic fictions” (page 399), “strategical assertions about marginality” (page 405), and “the rhetoric and activities of opposition” (page 411). This sounds so like the “dramatistic” perspective of Kenneth Burke that I am amazed Moore seems not to be influenced by Burke. This is doubly amazing because Moore confronts in this essay a central preoccupation of Burke’s career—the dilemma that words and ideas designed to handle one kind of issue tend to shift when the issue shifts. Burke’s language of “strategies” and “situations” embodies his contention that peoples’ beliefs must always be read in strategic context; this is, I think, an improvement on the characteristic historian’s tendency to offer up snippets of ideas in detached quote and paraphrase.

As a critic, Kenneth Burke invariably reads words and ideas with a skeptical eye. He is skeptical not merely to unmask social injustice; rather, he believes that no one speaks out of a generalized intellectual vacuum, but always through emotional involvement in some particular problematic situation.⁷ Laurence Moore instinctively senses that. But he has not quite put together the dramatistic language to gain full leverage on his materials. Burke has developed such a language, with an anthropology undergirding it. Thus Moore, and American cultural historians, could do themselves a favor by appropriating the intellectual bases and methodological tools of this dramatistic approach. In so doing, they might help build a bridge from history back over to the discipline of literature; such a bridge was once (in the 1940s and 1950s) strong and wide, but it has virtually been dismantled in the social scientific and politicalist onslaught of the last two decades.

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⁶ See Trilling, “Reality in America,” 1–19; Lewis, *The American Adam: Innocence, Tragedy, and Tradition in the Nineteenth Century* (Chicago, 1955), 1–10; and Bowron *et al.*, “Literature and Covert Culture,” *American Quarterly*, 9 (1957): 377–86.

⁷ For Burke’s most comprehensive statement of his position, see his *The Philosophy of Literary Form: Studies in Symbolic Action* (rev. edn., New York, 1957).

Reply:

EDWIN S. GAUSTAD AND GENE WISE are polite men, and I naturally appreciate the generous tone of their comments. But they do seem to share a serious doubt about whether my essay, however provocative, lands successfully on any readily usable point. Professor Wise begins, "Moore's essay raises more questions than it answers" (page 416); and Professor Gaustad writes, "Nothing is wrong with getting rid of bad terms, of course, but, if one consequence of so doing is that historians will be even more skittish about any venture into the thicket that is religion, then more has been lost than gained" (page 414). They are wondering, I think, "Even if we agree with many of Moore's observations, what are we going to do with the argument?"

I have, of course, an easy way to respond, which is to cling to the "fortunately" that Professor Wise uses to modify his first sentence. Since the essay is preliminary to a series of essays I am writing about American religious history, I do not really mind if my *Forum* piece is judged more provocative than conclusive. Still, I suspect that readers will tolerate inconclusiveness only if they are persuaded that the essay has addressed important problems that have been inadequately treated, indeed scarcely recognized, in the writing of American history. Therefore, in this reply, I should like to attempt a bit more than the easy response and try to clarify the essay's overall thrust in responding to some of the specific matters raised by my two critics.

ALTHOUGH PROFESSOR GAUSTAD UNDERSTANDS ME very well, he has nonetheless misread me in a few important places. For example, I did not mean to hold up Fundamentalism "as a unique instance wherein historians have misjudged importance, numbers, influence, and so forth" (page 414). Quite the contrary. The examples he adds are all well chosen. (If we are making lists, however, I should put in a word for what may be the most seriously overlooked and underestimated groups in the standard surveys of American religious history—the Disciples and the Churches of Christ.) The only caveat I wish to recall from my essay is that we cannot understand these omissions only by considering the biases of historians and the blind spots in our contemporary media coverage of religion. We must also consider how the omissions have been encouraged by the words and deeds of historical actors.

In saying that, with all of the attendant arguments about how past and present strategies may cause historians to apply labels carelessly, I did not intend, as Professor Gaustad suggests, to "debase" insider-outsider terminology "to such an extent that it can no longer circulate among historians" (page 414). With whatever success, I wanted to argue that insider-outsider disputes form a central theme in American history. At one point I even suggested that a proper understanding of how such disputes work may give us some clues about the relative stability of American society and politics. The problem is not that the terms bear on nothing

real (historians might even find an appropriate way to use the term “mainstream”) but that the biases that influence historians render them insensitive to the ambiguous meanings created by those who used those and similar terms in the past. I did indicate that historians in making purportedly objective statements about what was or was not “dominant” American culture quite commonly write nonsense. But I do not think that the essay ended on that discouraging note.

Professor Wise makes a number of interesting points, but I only have space to comment on his major ones. He suggests that “Consensus history” is in general a misleading term and that it has in particular steered me away from what I might have learned from, among others, Lionel Trilling and Kenneth Burke. I find Professor Wise’s term “counter-Progressive,” as an alternative to “Consensus,” an interesting one; and I have learned much from his own book.¹ I do not, however, think that the distinction he draws, even if one accepts it, makes much difference to my argument. I may have seized on familiar categories in order not to get too far afield in my remarks on historiography, but “consensus” is the most important feature of a certain type of historical writing I needed to analyze.

To be sure, “Consensus” is not a very full label to apply to the complex minds of Richard Hofstadter and Reinhold Niebuhr. If biography is relevant in estimating the shape of my argument, I happily concede that I have been strongly influenced by those men and by many others who figure in Professor Wise’s discussion of “counter-Progressives.” That my essay contains paradoxes and ironies and that it emphasizes “language as strategy” cannot be accidental. If, however, what I have called “Consensus history” abounds with ironies and ambiguities, if it typically denies Manichaean conflicts and shows how good intentions are always going awry in a flawed world, it locates the strengths and weaknesses of the American system in its ability to hold a steady and central course. For the purposes of my analysis, I was interested in how that point of view, which most certainly had implications for post-World War II politics, affected the historical depiction of insiders and outsiders.

Readers can go back to my essay to see what I have written on this point. But I might add a small example to illustrate the point where I start quarreling with Consensus narrative. At the beginning of his undeniably marvelous essay, “The Paranoid Style in American Politics,” Hofstadter wrote that he wanted to demonstrate “how much political leverage can be got out of the animosities and passions of a small minority.”² From my perspective that question, however rich with ironical implications, is just too loaded. One is likely to get farther by asking how political advantage may redound to a group that regards itself and is regarded by some others as “outsiders” but whose perception on this matter bears no unambiguous relation to its actual numbers or its actual animosities when compared to groups with an opposite self-perception.

Professor Wise is right to remind us of how much historians can learn about language and its uses from literary critics. But I cannot quite share his enthusiasm for building “a bridge from history back over to the discipline of literature” (page

¹ Wise, *American Historical Explanations: A Strategy for Grounded Inquiry* (Minneapolis, 1980).

² Hofstadter, *The Paranoid Style in American Politics and Other Essays* (New York, 1965), 3.

419)—particularly if that bridge is going to be the same one that Henry Nash Smith and R. W. B. Lewis erected. I think that I see why Professor Wise finds Lionel Trilling, who was interested in artists who contained within themselves a complicated cultural dialectic—"the yes and no of their culture"—relevant to my argument.³ Yet it is hard for me to imagine Trilling ever taking much interest in many of the people who most interest me. Literary critics are quite naturally and quite appropriately drawn to powerful minds. Although social scientists have no doubt caused intellectual and cultural historians considerable pain in insisting that the stories of Hawthorne and James have severe limits as historical documents, their "onslaught" has convinced me that the cultural dialectic that interested a past generation of American Studies proponents left out a lot of American experience.

LET ME COME AT THE GENERAL THRUST of my essay in another way. A colleague of mine suggested that my constant use of the word "narrative" in the essay is almost everywhere misleading. Bancroft, Prescott, and Parkman wrote narrative history, but most twentieth-century professional historians have concerned themselves with interpretation rather than with the artful telling of a story. Because of the modern analytical emphasis, I should not have suggested that a problem lies in the particular ways that American historians construct narrative. It lies, rather, in the ways they construct analytical, interpretive categories.

I have worried a good bit about this criticism and recognize that I may have made the word "narrative" stand for too many things. On balance, however, I remain convinced that narrative is a useful word for what I want to argue. A narrative or story line may be implicit and deeply submerged in historical writing, but, even in these days of methodological sophistication, it determines a great deal about how historians make sense of the past and about how they decide which groups and individuals merit historical attention. I realize that trying to decide whether a narrative line determines interpretation or vice versa is somewhat akin to the old puzzle about chickens and eggs. But surely the failure of historians to make a plot apparent to nonprofessional readers does not prove the irrelevance of narrative assumptions to their analyses. It only means that Barbara Tuchman and David McCullough have less competition on the popular market.

No doubt when I say "narrative," I often have in mind a rather grand pattern of events that transcends the who, what, and when of particular episodes. For instance, if I may continue to cite examples from the field of religious history, I have sometimes asked why it took American religious historians so long to escape the influence of their nineteenth-century predecessors, in particular the influence of Robert Baird, Philip Schaff, and Daniel Dorchester. Long after the biases of those men had gone out of fashion, the American churches to which they paid the most attention by and large remained at the center of American religious histories. I can think of many reasons for this, but high on the list I would place the fact that

³ Trilling, "Reality in America," in his *The Liberal Imagination: Essays on Literature and Society* (New York, 1950), 9.

those Protestant writers had a strong narrative line, underwritten by Providence, which pointed toward the eventual triumph of a few Protestant churches. It has been—and is—one thing to expose bias and omission in their Providentially guided plots and quite another to find a replacement that can so easily settle questions about what kind and amount of attention to shower on particular religious groups.

Whether we have in fact found such a replacement is an issue I must leave for another discussion. What I want to emphasize for present purposes is that we cannot give historical importance or meaning to purportedly neglected groups merely through the principle of equal time—that is, by making sure that survey texts give space to them. Historians can devote a hundred pages to Pentecostal groups but not change much about how we perceive the past unless the logic of the narrative suggests reasons why they form a central part of the story of American religious experience.

I only repeat myself when I say that “outsiders” cannot be given a central part, one that will encourage sustained historical attention and will generate important interpretive controversy, if narrative keeps them “objectively” excluded from a “dominant” culture and notices them only when they are judged to be exercising a healthy or progressive influence on that culture. In one of his own fine books, Professor Gaustad observed, “Throughout so much of American history, it has been distressingly difficult to separate the ‘ins’ from the ‘outs.’”⁴ I do not mean to denigrate the value of the leads I have gotten from Professor Gaustad and others in saying that American historians have not yet gone very far in exploring the fascinating interpretive challenges posed by this observation. For instructions about how to proceed, Professor Wise has appropriately reminded me of Kenneth Burke’s preoccupations with reading words as strategies for encompassing specific situations and needs. I may only “instinctively sense” what Burke understood clearly. But I suspect that for both of us, and for anyone else who is persuaded that something important is at stake here, the difficulty comes not with the knowing but with the applying of what one knows.

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⁴ Gaustad, *Dissent in American Religion* (Chicago, 1973), 3.

Reviews of Books

GENERAL

ELDON J. EISENACH. *Two Worlds of Liberalism: Religion and Politics in Hobbes, Locke, and Mill*. Chicago: University of Chicago Press. 1981. Pp. x, 262. \$20.00.

This is an original essay that revises significantly our usual views of the history and essence of liberal political thought. Eldon J. Eisenach had the good idea of reading the writings on religion of the three founding fathers of liberalism—Hobbes, Locke, and J. S. Mill. Most previous historians have dismissed these writings—and especially the second half of *Leviathan*—as unimportant asides or as smoke screens designed to hide the authors' unorthodox views from hostile Christian critics.

In fact, Eisenach claims, none of these three men, usually described as secular thinkers, thought it possible to construct theories of political obligation based on reasoning alone. They all recognized that beliefs that are neither logically provable nor empirically demonstrable are necessary cornerstones of a just and stable political order.

Hobbes is the key figure with whom the story begins. Locke and Mill restate, qualify, and amplify Hobbes's basic premises.

According to Hobbes, man is born into two worlds, one of which is a world of freedom and the other a world of obligations. The first world is the state of nature, a hypothetical world invented by philosophers and, in modern terminology, behavioral scientists, as the starting point for constructing political structures that can rightfully claim the obedience of citizens. This state of nature is an unchanging and abstract world of independent individuals who are free to do as they desire and who live in the environment of the natural scientist, where everything happens in accordance with the laws of nature. It is a world without history, cultural traditions, or religion.

But men are also born into a second world with a history, cultural institutions, superstitions, and religious sects, where men are motivated by beliefs in a supernatural power that rewards virtue, punishes evil, and explains things that men cannot fathom by reason alone.

Eisenach demonstrates persuasively that all three of his major liberal thinkers thought that the sanctions that exist in the second world were necessary to the creation of stable government. Hobbes and Locke went so far as to say that a belief in an afterlife with rewards and punishments was essential. Mill, who learned from reading history the importance of the irrational in human life, substituted a "religion of humanity," with its intuitive prophets like Coleridge, for traditional religion.

Eisenach concludes that his analysis of the liberal tradition has the virtue of destroying the view that social science and analytical philosophy now embrace all the knowable subject matter of politics and are the culmination of systematic political thinking. A true history—and defense—of human freedom must take into account the humanist and historical traditions.

Unfortunately, Eisenach's presentation is so intricate and terse that many pages are barely intelligible to the mere historian. It will likely find fewer readers than it deserves.

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New Brunswick*

MARJORIE O'ROURKE BOYLE. *Christening Pagan Mysteries: Erasmus in Pursuit of Wisdom*. Buffalo: University of Toronto Press. 1981. Pp. xiii, 174. \$15.00.

In this book Marjorie O'Rourke Boyle continues an argument from her earlier book, *Erasmus on Language and Method in Theology*, published in 1977. There Boyle noted that Erasmus's rhetoric and grammar are inextricably linked with his theology. Here she goes on to show through an analysis of his *Anti-barbari*, his *Praise of Folly*, and his colloquy "Epicureus" that Erasmus saw the classical authors not only as forerunners, but also as actual builders of basic Christian notions. Distinguishing her view from others, she says, "It is one theological idea that Christians may perceive that classical learning, with its profoundest truths about God borrowed from Hebrew Scripture, is their inheritance which they may adopt and convert to the glory of Christ. It is

another idea entirely that Christ himself has ordained that pagan learning to his own glory and that Christians must appropriate it if they are to be faithful to the divine economy" (p. 11).

The book has two major virtues. First, the author's evident love of Erasmus suffuses it with an empathetic tone and results in distinctly Erasmian turns of phrase as well as an occasional flash of Erasmian wit. Second, by stressing the religiosity of the humanist tradition, she reinforces a theme that has grown increasingly predominant in the literature since the publication of Charles Trinkaus's *In Our Image and Likeness* over a decade ago. After the long period in which the secularism of the Renaissance was assumed, it is important to apply this insight as fully and effectively as possible.

The book is strongest where the author is concentrating most firmly on Erasmus's success in using grammar and rhetoric to inculcate religious virtue. In particular the richly allusive section on *Moria* breathes the spirit of approbation as she discusses the role of folly in medieval mystery plays, the Homeric notion that the poet is privileged to name the gods, and numerous other traditions that broaden our understanding of that inexhaustible work. The book is at its weakest when Boyle is attacking Luther. She seems to feel that Erasmus's championing of a properly pious attitude is an adequate answer to Luther's theological depth. Such an argument requires much more disciplined reasoning than the author furnishes.

The book seems a little short, given that none of its conclusions is strikingly original. Boyle's argument that Erasmus departed from the Augustinian position on the classics is certainly sound, but few would dispute that, despite those who have seen Augustinian currents in his thought. Similarly, few would maintain that Erasmus argued for a self-sufficient man in opposition to Luther's bondage of the will. She has to go back nearly twenty-five years to find a book that suggests that.

Finally, more careful editing would have improved what is basically an enjoyable book to read. Notes 25 and 285 in chapter 2 refer to one another, leaving the reader to speculate on what the reference might be. Notes 109 and 110 in the same chapter are reversed. Given the size of the book, it contains too many repetitions. Both a long phrase referring to an observation by Craig Thompson and a quotation from Socrates are found twice in the first few pages. Later on we are told twice of Erasmus's observation to More that the *Moria* is only "table napkins."

DONALD J. WILCOX
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JUDITH A. MERKLE. *Management and Ideology: The Legacy of the International Scientific Management Move-*

ment. Berkeley and Los Angeles: University of California Press. 1980. Pp. ix, 325. \$18.50.

Judith A. Merkle's theme is that the organizational structures surrounding and controlling machine technology in modern societies are not an inevitable outgrowth of advanced industrialism "but are in large part the legacy of a systematic and massive industrial engineering crusade, the international Scientific Management movement, carried out in the first two decades of the 20th century" (p. 1). Although she is absolutely correct in stressing the interactions between technology and society, her use of the term "crusade" gives the impression of a concerted and unified ideology, which is belied by her own evidence. What Merkle does best is show how scientific management, developing out of Frederick W. Taylor's attempt to apply "scientific principles" to the work process and the man-machine interface, developed into a complex set of thought and practices which could be—and were—utilized in the service of vastly differing social and political ideologies.

What had been a "philosophy of private business" in the United States was absorbed by its bitterest enemy and shaped the pattern for economic planning in the Soviet Union, moving from the organization of factory production to a generalized administrative technique. Although Soviet apologists claimed that scientific management did not exploit the workers in a socialist context, but only in a bourgeois society, the fact is that in the Soviet Union it became a "monstrous form of worker exploitation."

Although scientific management fitted into the Saint-Simonian tradition and into "the classic technology of power of the centralized state" in France, Taylorism was not altogether successful there because industrial and social organization militated against it. In Germany, where the social setting emphasized the authoritarian aspects of Taylorism, it was utilized in rationalizing war production during World War I and in developing cartels in the 1920s. The totalitarian economic system of the Third Reich was a direct descendant of this rationalization movement.

When scientific management was brought to Britain in the 1920s, the combination of tradition and pragmatism deriving from nineteenth-century British industry led to an emphasis on the cost-cutting side of Taylorism rather than its output-maximization side. Again the introduction of Taylorism into a different social and institutional setting had quite different results from its use elsewhere.

In the United States scientific management later grew from a theory for the efficient organization of the machine shop into an ideology for managerial control of the state (Technocracy, Inc.) and New

Deal bureaucratization. Although scientific management's concept of efficient governmental organization might appear reactionary today, it seemed advanced when first enunciated: "The elitism of technical expertise seemed more democratic than the old federalist idea of the elitism of private ownership, and the promise of efficient government fairer to the citizenry than the favoritism and graft of the spoils system" (p. 289).

Merkle closes with the current and future dilemma of democratic control versus efficient organization in the management of technology. Her book sheds historical light upon this continuing problem of industrial society.

MELVIN KRANZBERG

Georgia Institute of Technology

A. J. H. LATHAM. *The Depression and the Developing World, 1914-1939*. Totowa, N.J.: Barnes and Noble or Croom Helm, London. 1981. Pp. 230. \$25.00.

This is said to be an extension of A. J. H. Latham's book on *The International Economy and the Underdeveloped World, 1865-1914* (1978), which I do not know. That may deal with the underdeveloped world; this deals only with parts of it, in Africa and the Far East, areas in which the author has worked and knows well. It was an understandable temptation to dress up a largely factual country-by-country description of the progress of colonies and newly independent states in Africa and the Far East as a general discussion of the developing world and to give it a thesis by asserting the critical role of those areas in the world depression. The performance, however, is not very convincing.

The first four chapters of this rather brief book deal with communications, money and capital (largely foreign investment), international trade, and population and migration in great and loving detail, profusely illustrated with numbers in text and in statistical tables. There are fourteen graphs in the book, forty-six tables in the text, and thirteen tables, covering eighteen pages, in the appendix. Trade is discussed with country and commodity breakdowns for selected years over the quarter-century, and full detail is provided on miles of railroads, census returns, and migration flows. There are useful treatments of such episodes as the Stevenson rubber plan and the fall in the price of silver that pressured Asian countries to switch from the silver to the gold standard.

It is hard to see, however, that this all adds up to a discussion of the developing world in the absence of attention to Latin America and the Middle East or, less certainly, to the regions of recent settlement such as Canada, Australia, and New Zealand, which might have been called "developing" in the 1920s. Copper cannot be adequately treated without Chile,

which is not even mentioned in the index, sugar cannot be treated without Cuba, nor coffee without Brazil, nor is a discussion of world oil adequate if it is limited to Burma, the Dutch East Indies, and India.

As for the explanation of the world depression as resulting from the overproduction of rice in Asia, which, added to overproduction of wheat, brought about a collapse in agricultural prices and incomes and a decline in agricultural demand for the output of the world's mines and factories, the analysis is sketchy. Latham asserts rather than argues his thesis, which is not ranged against alternative models, whether the simplistic ones that focus exclusively on the money supply, or the more complex ones that combine agricultural overproduction in many commodities, including sugar and coffee, with capital flows, exchange rates, speculation in securities, and liquidity seizures, not to mention overshooting production of durable consumer goods such as housing and automobiles. The author should bear in mind that up to the middle of the nineteenth century the favorite business-cycle model was the exact opposite of the one he subscribes to without much examination: prosperity was believed to come from bumper crops and depression from crop failure.

The book then is useful in its descriptions of the course of development of a number of countries in Asia and Africa but fails to live up to the promise of its title and flyleaf.

C. P. KINDLEBERGER

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E. L. JONES. *The European Miracle: Environments, Economies, and Geopolitics in the History of Europe and Asia*. New York: Cambridge University Press. 1981. Pp. ix, 276. Cloth \$29.50, paper \$9.95.

This book, insofar as it examines the environmental, economic, and geopolitical factors that encouraged "very long-term" economic change in Europe before 1800 while discouraging it in Asia, is an example of comparative, all-encompassing history *de rigueur*.

The work consists of five sections. The first, composed of two chapters, "Environmental and Social Conjectures" and "Disasters and Capital Accumulation," considers the peculiar and much less stringent European pattern: a less hazardous environment, greater population restraint, and security of life and property worked in Europe's favor. The second section of five chapters inquires into the distinctive traits of European technology, expansion, the market economy, the states system, and nation-states. Prominence is given to the novel nature of Europe's rise and the effect of the windfall gains resulting from Europe's expansion overseas. Europe was doubly fortunate: it enjoyed the advantages of political decentralization as well as the

benefits of a common culture and economy—one complementing the other. The third section, composed of the single chapter "Beyond Europe," is followed by a fourth that provides an account of developments in "Islam and the Ottoman Empire," "India and the Mughal Empire," and "China and the Ming and Manchu Empires." (Japan is deliberately omitted.) The final section is headed "Summary and Comparison." All in all, especially in these days when generalists are out of favor, this book is an engaging tour de force.

A study of this kind, however, cannot avoid being episodic and fragmentary. Synthesis and summarization to this degree must entail oversimplification. In E. L. Jones's text there are too many leaps in the narrative as well as in the dark; too many demands are made on the reader's credulity. Population figures are given (p. 155) for Africa from 10,000 B.C. as if they were gospel truth; "China," it is said (p. 160), "came within a hair's breadth of industrialising in the fourteenth century." To try to explain population trends in China and India (p. 15) by saying that "copulation was preferred above commodities" is unhelpful to say the least. Ritual in warfare (p. 35) was confined neither to Europe nor, within Europe, to military life. If the Crusades (p. 53) were an example of "would-be mass outmigration" then how do we explain the Children's Crusade? There is too much here of the assumption that Asia should have developed our way but did not; too little appreciation that what Jones is investigating in Asia is often an *alter orbis*. Was the history of Eurasia before 1800 as simple, as certain, as sequential, and as comparable as this? I do not think so. But then I doubt there is a scholar in Eurasia who could do justice to a theme of this magnitude. Jones is a persuasive writer, but on this scale he can also be misleading.

The author's assertion (which substantiates the work of others) that Europe's achievement of sustained economic development leading to industrialization was dependent on novel elements rather than upon ordinary, continuous, and universally applicable elements has far-reaching implications. It suggests that a good deal of the recent debate about the process of economic growth and development has been based upon false historical analogy—a sobering thought for the poorer parts of the world.

WILLIAM WOODRUFF
University of Florida

ANCIENT

BENNETT SIMON. *Mind and Madness in Ancient Greece: The Classical Roots of Modern Psychiatry*. Reprint. Ithaca: Cornell University Press. 1980. Pp. 336. Cloth \$19.50, paper \$6.95.

Bennett Simon's work should earn the respect of a good many psychiatrists, psychohistorians, classicists, and ancient historians. In clear, readable prose (admirably free from psycho-jargon), he succinctly describes contemporary models of mind and mental illness as a prelude to analyzing ancient Greek descriptions of mental life. Relying primarily on the evidence of Homer, the tragedians, Hippocrates, Plato, and Aristotle, Simon constructs three models of mind—poetic, philosophic, and medical—and analyzes the description, explanation, and treatment of mental illness in each.

According to Simon's poetic model, people in the Homeric Age conceived of mental activity as initiated by external agents and sustained through a "series of interchanges" among the parts of an individual; both aberrant behavior and relief from emotional distress were also caused by external forces. People of this era did not think of themselves as separate, autonomous individuals.

With the birth of philosophy and rational-empirical thinking in the late archaic and early classical periods, the poetic model was modified and Simon's other two models appeared. A recognized concept of "self" appeared in all three models. The modified poetic model persisted in the work of the tragic poets; human characters were capable of initiating and sustaining their own mental activity, but only the gods could drive them mad. The philosophic model posited a mind-body dichotomy, in which the normal mind was hierarchically structured and governed the body; mental illness arose from either a conflict within the structure or ignorance of what the mind needed to know for harmonious activity. The medical model also posited a mind-body dichotomy but looked exclusively to physiological phenomena to explain such mental illnesses as melancholia or hysteria.

Simon often draws parallels between the ancient models and modern theories, linking, for example, Plato's "baser" parts of mind with Freud's "unconscious," and Aristotle's black bile with biochemical dysfunction. This linkage illumines both ancient and modern thought about the mind. Moreover, it shows that despite radical differences in the way ancient Greeks and modern psychologists describe mind, there are striking commonalities in their fundamental conceptualizations of the way the mind works. Particularly interesting are Simon's suggestions about the different ways in which epic poetry, tragedy, and philosophy served as therapy for both creator and audience.

However illuminating and suggestive, Simon's work does have some shortcomings. (1) By relegating nearly all discussion of female emotional distress to a chapter on "Hysteria," Simon sexualizes his study: only men can be mad; women are simply hysterical. Those sensitive to feminist issues will be

dismayed to read that "Aeschylus' *Oresteia* ends in a triumphant reconciliation of male and female . . ." (pp. 109–10); Athena's judgment that the mother is no true parent of the child hardly reconciles the sexes; it subordinates women. (2) Simon's analysis of mental activity is limited to humans; he does not consider descriptions of the mental activity of the gods. This omission is especially serious in his treatment of Homer; in the epics it is true that men do not autonomously initiate and sustain thinking, but the gods do. Thus, one cannot say that Homeric Age Greeks did not conceive of an autonomous mind. (3) Occasionally Simon takes for granted the validity of some aspects of psychoanalytic theory that are not, in fact, generally accepted by psychologists, let alone classicists and ancient historians. His argument, for example, that the "core unconscious meaning of madness in the Platonic dialogues" reflects Plato's primal scene fantasies will strike some as outlandish (p. 174).

Despite these flaws, Simon's work deserves to be taken seriously by anyone interested in the way ancient Greeks thought. It is firmly grounded in classical scholarship, clearly argued, and usually judicious.

VALERIE FRENCH
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T. LESLIE SHEAR, JR. *Kallias of Sphettos and the Revolt of Athens in 286 B.C.* (Hesperia: Supplement, number 17.) Princeton, N.J.: American School of Classical Studies at Athens. 1978. Pp. x, 117. \$10.00.

In 1971 the excavators of the Athenian agora came upon a marble stele with an unusually long inscription of one hundred and nine lines (Inv. No. I. 7295). Although the stone had been reused as a cover slab for the great drain, the lettering was still almost perfectly intact and proved to be a decree voted by the people in 270–69 to record the courageous deeds of Kallias of the deme Sphettos and to pay him honors. Heretofore, Kallias was known simply as one name among several on a routine inscription from Delos, but now it emerges that he was a member of a wealthy Athenian family that for three generations played a vigorous part in the public life of the city.

His grandfather had been elected general at least three times beginning in 347–46. His father had fought on the side of Antigonos the One-Eyed in 316. Kallias himself, for reasons unknown, subsequently left Athens either voluntarily or as an exile, so that we come upon him rather abruptly in 286 while he was commanding a Ptolemaic garrison on the island of Andros. In that year Athens rose against the occupation force of Demetrios of Macedon, and Kallias brought help in the form of a thousand mercenaries maintained at his own ex-

pense. With the assistance of these troops, the Athenians drove Demetrios's soldiers out of the city. Kallias and his men then helped hurriedly to bring in the harvest and to prepare against the besieger's inevitable counterattack. When it was delivered, Kallias went into action once more and was wounded in the fighting. Demetrios was forced to withdraw and to concede Athens a favorable peace. For the next ten or fifteen years, Kallias acted as mediator between his city and the first two Ptolemies, and in one emergency brought the city fifty talents of money and the huge amount of twenty thousand measures of grain. For all these invaluable services he was given a number of honors, the usual one of a seat in the front row at the contests and the unusual ones of a gold crown and a bronze statue in the market place.

All this is new. The record of Kallias's *aristeia* serves to confirm and flesh out the scrappy remains of Athenian history in the early third century that Plutarch and Pausanias have preserved. We now know that Ptolemaic intervention in the Aegean against Macedon began under Soter and not under Philadelphos.

The present monograph contains the Greek text of the inscription, a translation, a long and careful commentary, and a thoughtful discussion of the revolt of Athens. It is a contribution to hellenistic history.

SAMUEL EDDY
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JOSEPH FONTENROSE. *The Delphic Oracle: Its Responses and Operations with a Catalogue of Responses*. Reprint. Berkeley and Los Angeles: University of California Press. 1981. Pp. xviii, 476. \$10.95.

A paperback edition of Joseph Fontenrose's study is welcome, and there is no need for a review of detail (see for example F. E. Brenk, *Gnomon* 52 [1980]: 700–06; B. C. Dietrich, *AJP* 101 [1980]: 234–41). The accounts that he gives of the nature and transmission of oracular responses and especially of mantic procedure are clear, thorough, and sensible, among the best we have, and the general air of skepticism that he breathes over them nothing if not healthy. Delphic chasms and vapors and frenzies should have been dead a long time ago (at least since P. Amandry, *La mantique Apollinienne* [1950]), but it is as well to confirm the death, particularly if it is done with skill and authority.

But most interest will focus on Fontenrose's analysis of modes, topics, formulas, and occasions of surviving oracles and on his catalogue of them (including those of Didyma). The factual content and the collection of evidence will be of inestimable use to any student of oracular affairs. The comparison of "quasi-historical" responses with on the one

hand "historical" and on the other "legendary" responses is, to me, a new venture and a fruitful one. I am not sure that the same can be said of the conclusions.

There are two main points. First, the statistical results are not nearly so striking as Fontenrose claims. They are not so striking in themselves and cease to be striking in the least when we pause to take into account the difference in the nature of the evidence for the two extremes. Literature does not tend to record the unembellished, the uncolorful; chanceries do not spout romance. Nor is Fontenrose always persuasive when he tries to delete some of the apparent exceptions to his characterization (Thuc. i 118.3 is a forecast not a command).

Second, an objection that Fontenrose raises only to brush aside, contemporary Greeks believed that Delphi behaved in the ways that historians claimed to record and poets put in fancier dress. Contemporary Greeks knew how Delphi behaved, so did the historians, who were not all falsifiers, so did the poets who should have been aware that poetic license had its limits. We should be rash to deny that there was anything at Delphi to justify the belief, the representation, or the permissible misrepresentation.

Finally, an argument that Fontenrose does not counter. There are some areas of Delphic activity, topical or chronological, in which the surviving "genuine" responses—"genuine" as judged not by Fontenrose's austerity but by something like H. W. Parke's moderation (*The Delphic Oracle* [1956])—provide a pattern of Delphic behavior that accords uncannily well with modern interpretations of Greek history that the Greeks themselves did not have or did not make explicit either through lack of information or lack of technique. The Greeks recorded oracles without much discrimination (in matters of content of course much is palpably false); Parke has applied the discrimination but is chary of building on his own foundations. Those a little less cautious find that Parke "works." Fontenrose does not.

Fontenrose forecasts in his preface that his conclusions will meet with objections. These are some—but they are offered with gratitude and admiration.

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JOAN M. FRAYN. *Subsistence Farming in Roman Italy*. London: Centaur Press. 1979. Pp. 168. £8.50.

This short work provides an absorbing overview of the day-to-day life of certain types of peasants in rural Italy during the late Republic and early Empire. Some of its best chapters provide a vivid account of the working conditions and activities on

small farms as well as useful descriptions of plants, the physical environment, farm implements, and farm dwellings. The title is misleading, however, in so far as it suggests that the book provides a thoroughgoing examination of subsistence farming as a social, cultural, and economic way of life. Although in some ways this is a pioneering work, Joan M. Frayn restricts herself to a narrow, almost antiquarian point of view.

The distinction between subsistence farmers and other kinds of small farmers is either blurred or not considered at all. The literary sources, which derive almost exclusively from the late Republic and early Empire, speak of a wide variety of smallholders, but there is no clear way of distinguishing among them, and Frayn uses such terms as peasant, subsistence farmer, smallholder, and small farmer almost interchangeably. Yet it is evident from the author's own discussion that there are important differences between these categories of agriculturalists. The colonial smallholders discussed ably in chapter 6, for example, will have had little in common with the marginal peasants of the remoter districts of Italy discussed elsewhere in the book.

The failure to make these distinctions contributes to another set of problems. Although the author does not explicitly address the issue, she seems to assume that prior to the advent of the new style agriculture of the second century B.C. Italian farmers consisted principally of subsistence-level tillers of small plots. With the advent of villa and latifundia-style agriculture, these subsistence farmers retreated to the back country where they appear in later Latin literature as picturesque but genuine reflections of the past. Apart from the complicated question of the disappearance (if that is what it was) of the peasantry in the second century, this is a questionable assumption. Even prior to this date it is probable that farming in Italy was a comparatively diverse affair, and inferences drawn from later sources regarding subsistence farming (or even agriculture in general) in the earlier period will be tenuous at best.

The omissions of the book also reflect the narrowness of its underlying conceptualization. While the author wishes to avoid repeating what can be readily found elsewhere and sensibly proposes to focus on the daily activities of the peasants, it is hard to understand why there is no discussion of what might be termed the structure of peasant life. Religious beliefs and events, levels of political consciousness, forms of entertainment, and communal efforts of all kinds, including labor-sharing activities, are not touched upon. Attitudes toward production, which have been much debated in the general literature on peasant history, as well as the way peasants coped with the impact of cash crop farming have also been neglected. More could have been

made of what the surface surveys have to say about site selection, distribution of settlement, and networks of communication than is suggested by the brief references that are made to these useful sources of information.

These shortcomings should, nevertheless, not obscure the valuable contributions made by this book. Frayn's comprehensive knowledge of the sources and her familiarity with the Italian countryside is reflected in her fascinating discussion of aspects of daily life on the small farms of Roman Italy. There is great satisfaction in discovering new and concrete meaning in familiar literary references. Our still very rudimentary understanding of life in the Italian countryside in the Roman period has been advanced considerably by Frayn's thoughtful research.

D. BRENDAN NAGLE
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RONALD MARTIN. *Tacitus*. Berkeley and Los Angeles: University of California Press. 1981. Pp. 288. \$30.00.

Tacitus has been well served in the last quarter century. Ronald Syme's monumental two-volume study stands alone, still dominating the field. But several books, less ambitious in intent and scope, attempt to distill the greatness of the historian and to explain his place in the affairs of the principate and in the development of Roman historiography. Ronald Martin's *Tacitus* is one of the best.

Martin has been concerned with his author-subject for some thirty years and has for long been recognized as one of the keenest present-day students of Tacitean language and style. His sharp insight into nuance and his firm understanding of Tacitean virtuosity inform the book throughout. Martin's purpose is to present a straightforward picture of his subject, with little scholarly debate, although that is not totally absent from the footnotes. His judgments are sound and well balanced; only rarely would I argue with him. Coverage of important material is generous and accurate, although I did miss mention of the problems on the name of the Germans and the expression *urgentibus imperii fatis* in the section on the *Germania* and of Corbulo's *Memoirs* as a likely source in the discussion of the sources for Claudius's reign. And his publisher has done well; the production of the book is almost impeccable, with only a few trivial misprints.

There are eleven chapters, one each on the tradition, Tacitus and the contemporary scene, the lesser works, and the *Histories*, three on the *Annals*, two on the sources of the major works, one on style, and finally a postscript. In approach and coverage Martin's book is comparable to my own, *An Introduction to Tacitus* (1975).

Why has the reputation of Tacitus risen so dramatically in the last few decades? Martin suggests a sound answer: "the quality that sets him apart from other Roman historians is his ability, while living within, and actively serving, the Principate, to look at the history of the past hundred years with the eye of a Republican writer" (p. 234). There is thus a paradox in his life; his awareness of that paradox, "unique among his contemporaries, is perhaps one of the chief reasons why the writings of Tacitus make so immediate an appeal to our own age" (p. 38). Martin's book elegantly and informatively explains his subject's greatness.

HERBERT W. BENARIO
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ANTHONY R. BIRLEY. *The Fasti of Roman Britain*. New York: Clarendon Press of Oxford University Press. 1981. Pp. xii, 476. \$85.00.

The Roman provinces were administered by governors sent out from imperial headquarters in Rome in a regular pattern that followed long-established customs and rules. In the so-called senatorial provinces, that is, those left to the senate to administer, the governors were called proconsuls and they remained only one year in office, while in the imperial provinces they were chosen directly by the emperor, were sent to those provinces that he retained under his own supervision, were called *legati Augusti pro praetore* to indicate their position as deputies of the emperor, and remained as governors usually for four or five years, sometimes even longer at the discretion of the emperor.

The bulk of the armed forces was stationed in the militarily more important imperial provinces. All these governors were career officers, spending most of their adult lives in the imperial service, rotating from military to civilian posts. In order to reach the height of their careers they inevitably had to obtain the favor of the emperor, or at least win his respect. Thus, a roster of governors and other personnel in the various provinces will enable us to discover the men who made the whole system function. They did the work of governing the huge provincial complex, administering justice, keeping the peace, waging war, and so forth. Their careers are true reflections of how the Roman world was organized and administered. Our sources fortunately are full enough to allow us to reconstruct their careers, discover their social or ethnic origins, and fill out the provincial histories of many areas of the empire. Much excellent work has already been done on most of the provinces, and Anthony R. Birley's new book will rank among the best of them.

Roman Britain was an imperial province with governors of high (consular) rank. Birley has assembled in chronological order all its known governors

and military personnel of officer status. For each of them he cites and comments on the extant sources, quoting most of them in full. His judgment is always sound and reasonable, his reluctance to go much beyond the sources commendable. For the first one hundred and fifty years or more of its existence (A.D. 43 to the very early years of the third century) Roman Britain formed a single province under governors of consular rank with large legionary and auxiliary forces under their orders. Then, at least in A.D. 220 and probably just a few years earlier, it was divided into two provinces, an upper and a lower Britain. Several problems remain unexplained, however, about the actual arrangements made at that time concerning the location of the legions and the governors. Later, when the Verona List was compiled (ca. A.D. 312–14), Britain had already been divided into four provinces, all of them forming a single *diocesis Britanniarum*. Exactly when this second division of Britain took place is not known. And sometime later in the fourth century a fifth province was created. By that time, of course, the titles of the governors had been changed and they themselves came from entirely different social backgrounds. In addition, the military commands had been withdrawn from the office of the governors and placed in the hands of *duces* and *comites*. Birley cites all the evidence for these changes, displaying great familiarity with the many problems of interpretation and consequent controversy. After the Roman troop withdrawals from Britain by Emperor Constantine III in A.D. 407, there is no longer any evidence for continued Roman administration, and thereafter the island was left to work out its own future by itself. On the mainland huge, uncontrollable forces were at work gradually transforming the great empire into a series of Germanic kingdoms. Britain awaited its turn.

The reviewer finds this book a model of its kind and hopes it will stimulate even further work on Roman Britain and also on that dark age after the departure of Roman arms and the arrival of the Saxons. Although written with the specialist in mind, it may be used profitably by all who are interested in early Britain and its administration under Roman rule.

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Z. RUBIN. *Civil-War Propaganda and Historiography*. (Collection Latomus, number 173.) Brussels: Latomus Revue d'Études Latines. 1980. Pp. 246. 1,000 F.

This work is less comprehensive than its title might suggest. In fact, Z. Rubin discusses the supposed influence of the propaganda of Septimius Severus, emanating from his contests with political rivals, on

three writers of the time, Cassius Dio, Herodian, and Marius Maximus. After a short introduction and a chapter on "Some Basic Facts about Severus' Propaganda," the bulk of the work consists of three chapters, each devoted to one of these authors, analyzing their accounts incident by incident to demonstrate the reflection in them of imperial propaganda. A brief epilogue follows, as well as two appendixes, one on the imperial acclamations of Septimius Severus and the other on Herodian's knowledge of Alexander the Great. There is a general index and index of modern authors, but no bibliography.

The difficulties of Rubin's task are considerable. First, there is the character of the writings studied: Cassius Dio survives for the reign of Severus only in a late epitome, Herodian is widely recognized to be inaccurate and rhetorical, and Marius Maximus has to be reconstructed from the *Historia Augusta* and other late writers. Second, Rubin depreciates the evidence of Severus's coinage and imperial monuments, neither of which is studied systematically, and cites no other evidence except the writings for his analysis of the nature of Severus's propaganda. Lack of an objective, external control creates the risk of circular argument since the propaganda is recreated from the writings studied; there is also a tendency to interpret all remarks about the emperor as reflections of Severan propaganda or reactions against it. Third, the earlier writings of Cassius Dio, whose tone was favorable to the emperor, have to be reconstructed from his later work. Finally, the curious nature of the *Historia Augusta* and the diversity of views about it necessarily make it difficult to use its evidence with complete assurance, although Rubin himself is firm and clear about how he regards it.

Rubin is admirably fair about the limitations of the evidence and many of his suggested interpretations are ingenious. The detailed discussion of individual passages, however, is likely to prove heavy going for any but those already initiated into the study of the period. A common weakness of source criticism, apparent here also, is the persuasion that the writer studied *must* derive any significant material from some predecessor. Rubin does not allow sufficiently for the influence of historiographical commonplaces (for example, see his discussion of weather miracles). Nor does he accord enough significance to rhetoric: he believes, for example, that the Alexander theme in Herodian's description of the battle of Issus is entirely owed to Herodian's source, since Herodian's history otherwise shows little knowledge of Alexander (p. 104). He neglects the prominence of Alexander themes in the rhetorical instruction that all writers of the time received.

In summary, the book offers an honest, detailed, and sometimes ingenious discussion of the literary

sources for Severus and Severan propaganda, but does not carry complete conviction, partly because of the nature of the sources and partly because of Rubin's narrow focus.

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WALTER GOFFART. *Barbarians and Romans, A.D. 418–584: The Techniques of Accommodation*. Princeton: Princeton University Press. 1980. Pp. xv, 278. \$25.00.

This book is a collection of interpretative and often technical essays. Although the argumentation is rather intricate, contrary in tone, and hypothetical, as a group these essays make a learned and positive contribution toward solution of many of the obscure problems of barbarian settlement in the western European regions of the Roman Empire in the fifth and sixth centuries. The initial chapter, "The Barbarians in Late Antiquity and How They Were Accommodated in the West" is a very useful broad survey. Chapter 2, "The Roman Basis for Barbarian Land Allotments" is very important: Walter Goffart persuasively criticizes existing assumptions about the relevance of Emperor Arcadius's law on quartering soldiers for understanding the problem of the assignment of portions of land to barbarians. He correctly emphasizes that Arcadius was concerned with the narrow question of soldiers' lodgings, not with long-term remuneration and maintenance; Arcadius's law does not appear to have been the origin of the land-sharing arrangements of various Germanic peoples in western Europe. Goffart's thesis in subsequent chapters that Germanic allotments originated in shares of tax assessments is much less certain. He admits that the evidence is "so rare" (p. 222) and that he "states a possibility rather than a firm conclusion" (p. 121).

Goffart has consulted the relevant modern literature (except one should cite the 1975, not the 1969 edition of Cérati) and the meager primary sources; the footnotes and five appendixes are erudite. Extant documentation, however, is insufficient to prove the theses of chapters 3 through 8 conclusively. In fact, the result of Goffart's investigations increases doubts about what was thought to be a firm foundation of scattered source materials. The only certainty that emerges is a continuing and probably increasing scholarly debate about the critical and correct interpretation of the limited evidence.

This is an important discussion of one of the most basic aspects of the transition from late antiquity to the Middle Ages, written by a scholar who has investigated problems of Visigothic, Burgundian, Frankish, and Lombard history since the inception of his academic career. These essays are indispens-

able reading for investigators of the *Völkerwanderung*: provocative and tentative, they vary in persuasiveness.

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MEDIEVAL

GEORGES DUBY. *The Age of the Cathedrals: Art and Society, 980–1420*. Translated by ELEANOR LEVIEUX and BARBARA THOMPSON. Chicago: University of Chicago Press or Croom Helm, London. 1981. Pp. v, 312. \$22.50.

The Age of the Cathedrals by Georges Duby is an impressionistic essay that seeks to identify the roots of Western medieval art. Although the book brilliantly evokes the mentality of the aristocratic society that inspired and supported the artists, the modern reader expecting Duby's customary tight reasoning and coherence may be disappointed. Nevertheless, the very broadness of intent places the work in the company of such studies as *Mont-Saint-Michel and Chartres* by Henry Adams.

Duby posits an important relationship between artist and intellectual throughout his period—a somewhat surprising thesis for an economic and social historian. He argues that the eleventh century embodied the ideas of monastic thinkers in Romanesque art, while the twelfth and thirteenth centuries, sparked by a theology of light and stimulated by the dialecticians, developed the Gothic style that became orthodox propaganda to combat threatening heresies. As the universities of the late thirteenth century grew increasingly sterile, Gothic art too lost its vitality. In the cultural boom of the fourteenth century, a new artistic realism reflected the ideas of Ockham. Art became secularized as its audience expanded, and although the patronage base began to include wealthy merchants, it continued to assert chivalric rather than bourgeois values. Indeed, the author stresses that even in the fifteenth century, when Florence and Flanders replaced Paris as artistic centers, art remained aristocratic. Duby closes with van Eyck and Masaccio who began the process of freeing the painter from patrons and intellectuals, thereby initiating a new artistic era.

This study was originally published both in French and English by Skira in 1966–67, as a three-volume set richly and profusely illustrated (374 plates). The current edition has been retranslated into English and printed more modestly in a single volume with thirty-five black and white prints. A little of the original text has been cut, and some paragraphs have been taken out of context and reassembled in an awkward fashion as commentary for the plates. One could wish that Duby himself had revised the study in order to take advantage of

new research done, for instance, on the status of women, and so that the reader would know whether changes of dates and subtitles were Duby's or the result of editorial tinkering. The translators have generally managed to preserve the quality of Duby's vivid prose, in which a commune becomes a "combat association" (p. 110) and the second storey of the Sainte-Chapelle is "an airy snare intended to catch every passing ray of light" (p. 148). *Frères*, however, is translated as friars for eleventh-century monks and the proofreaders overlooked such enigmatic sentences as: "With the irruption of lay practicing it proliferated again" (p. 231).

Despite the drawbacks of this edition, the study stands out as a provocative and illuminating work. The author is so thoroughly in tune with his period—particularly the earlier centuries—that the reader is plunged into the feelings of medieval men and thereby into the world that produced the cathedrals.

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WARREN T. TREADGOLD. *The Nature of the Bibliotheca of Photius*. (Dumbarton Oaks Studies, number 18.) Washington: Center for Byzantine Studies, Dumbarton Oaks. 1980. Pp. xv, 206. \$20.00.

This book is the outgrowth of a Harvard University doctoral dissertation in Byzantine Greek. Warren T. Treadgold undertook to clarify some thorny issues connected with Photius's *Bibliotheca*. This Byzantine work reputedly contains 280 analyses of books that Photius claims to have read. Treadgold has, however, arrived at a figure of 386 "books." These analyses are summaries and epitomes, but often Photius also appraised the book's style and content.

One reason why scholars have attached so much value to the *Bibliotheca* is that 211 of the books Photius read do not survive in as complete a form as he presented them and 110 are completely lost.

Of the two better-known texts of Immanuel Bekker (1824–25) and Rene Henry (1959–77), Treadgold prefers the latter. He also refers to the edition of Martini (1921), which contains the *pinax* that the other two editions have omitted. In addition to these three texts, Treadgold consulted microfilms of the more important manuscripts (Marcian 450 and 451) and concluded that "all the present editions of the *Bibliotheca* ought to be used with some caution."

Treadgold claims that "there is no scholarly consensus at present about the date, method of composition, or nature of the *Bibliotheca*." He himself believes that "much about how and why the *Bibliotheca* was written depends on its date and the circumstances under which it was composed." The views of

Orth, Hergenroether, Halkin, Lemerle, Mango, Severyns, Wilson, Haegg, and Kustas are briefly cited to explain the complexity of the problems.

Treadgold emphasizes that a sound investigation must start with the text of the preface and postface, which state that Photius composed the *Bibliotheca* shortly before he was to leave on an embassy to the Assyrians (Arabs?). Treadgold sets the date of composition sometime between 843 and 858. The embassy in question seems to have been one in 845 to Samarra (a former Assyrian city on the east bank of the Tigris, refounded in 836 to be the capital of the Abbasid Caliphate, renamed Surra Man Ra'a). His conclusion is that if Photius was appointed to the said embassy, the *Bibliotheca* was composed "informally and hastily" in the spring of 845.

In a chapter called "The Second Part (Codices 234–80)" Treadgold conjectures that "all of codices 234 through 280 . . . were copied by Photius's secretary from rough notes that Photius had taken before he composed the *Bibliotheca*." Treadgold explains that the first part of the *Bibliotheca*, codices 1–233, was dictated by Photius at the request of his brother Tarasius just before the embassy was to depart. His secretary copied the rest because Photius "was running short of time before his embassy was to leave."

The book has also a chapter on biographical sketches of the authors Photius read, which he believes may be properly called literary history. Georg Wentzel has already done research to identify them.

In chapter 5 of his book Treadgold discusses the errors and omissions of the *Bibliotheca*. The book is provided with a useful, annotated table of contents of the *Bibliotheca*, an author index to the codices, a subject index to the codices, an index to the codices by centuries of composition, an index to the codices by Tyjys, a general index, and a bibliography.

The book is carefully written, and from the nature of the subject it is obvious that it would attract chiefly scholars—Byzantinists and classicists who are interested in the transmission of the classical tradition.

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MALCOLM VALE. *War and Chivalry: Warfare and Aristocratic Culture in England, France, and Burgundy at the End of the Middle Ages*. London: Duckworth. 1981. Pp. 206. £18.00.

This useful and interesting volume has some important things to say about warfare and military values in the fifteenth century. Malcolm Vale's point of departure is Johan Huizinga's *The Waning of the Middle Ages*. Huizinga, who had a favorable notion of chivalry in its prime, saw it in decay in the

fifteenth century and thought that the gulf between chivalric ideals and the actualities of war had widened considerably. Vale questions this thesis, and his first chapter, "Literature of Honour and Virtue," makes the important point that literature addressed to the fifteenth-century military aristocracy called for the adoption of Roman values, rather than invoking those of the medieval chivalric romances.

A second chapter, "Orders of Chivalry in the Fifteenth Century," underscores the political importance of orders like the Golden Fleece that bound leading nobles to the sovereign prince. In a chapter on "Chivalric Display," Vale tries to show that the tournament was still regarded as essential military training as well as a part of the noble lifestyle. A fourth chapter, "The Techniques of War," deals with the place of cavalry, firearms, and armor. The concluding chapter discusses "The Changing Face of War and Chivalry." We learn that cavalry remained essential to winning a decisive victory on the battlefield and was especially useful when the enemy did not use field artillery. Firearms and the increased weight of armor finally doomed heavy cavalry in the sixteenth century, but the change did not eliminate lighter cavalry and certainly did not end the military role of the nobility. Nobles, in fact, proved quick to adapt to new techniques such as the use of firearms. They still made up a large part of the armies that acquired a new professionalism with the introduction of uniforms, insignia, better discipline, and the elimination of different pay scales for different social ranks.

Vale points out that individual prowess was one legacy of medieval chivalry to the Renaissance but that the cult of honor and fame derived from Roman writers was entirely compatible with Renaissance humanism. The greater discipline and reduced individualism in the army led nobles to seek a new outlet for expressing individual honor, and the duel became a significant activity in the sixteenth century.

Although he makes a valuable contribution to our understanding of the period, Vale does not really demolish Huizinga's picture of chivalry in decay. He does not mention that earlier chivalric virtue of "courtesy" in the sense of treating kindly those who are weak and defenseless—an idea that is prominent in the Arthurian stories but not in the Roman works preferred by the fifteenth century. If one treats this value as an essential component of medieval chivalry in its prime, then fifteenth-century chivalry may have lacked a quality of importance.

The appendixes, tables, and bibliography are useful parts of this book, and the bibliography is especially so since it contains materials from more than one academic discipline.

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MARJORIE CHIBNALL, editor. *The Ecclesiastical History of Orderic Vitalis*. Volume 1, *General Introduction, Books I and II (Summary and Extracts), Index Verborum*. (Oxford Medieval Texts.) New York: Clarendon Press of Oxford University Press. 1980. Pp. xvi, 386. \$98.00.

The appearance of volume 1 of Marjorie Chibnall's edition of the *Ecclesiastical History of Orderic Vitalis* completes a labor of a quarter of a century. Volumes 2 to 6, which have already been issued, make available a superbly edited Latin text and a unique English translation of the work of a major medieval historian. The bulk of the more original portions of Orderic's writing is contained in volumes 2 to 6 (Orderic's books 3–13). Volume 1 reproduces the prologue and books 1 and 2. An English translation is provided for the prologue, but for reasons of economy books 1 and 2 are printed only in Latin. Since they are composed primarily of passages harmonized from the Gospels and other familiar sources, the absence of translation will not seriously penalize the English reader.

Volume 1 is more significant for what it contains of Chibnall's work than of Orderic's. Much of it is devoted to a "General Introduction" and to a variety of scholarly aides to the text. Of particular interest is a lengthy "Index Verborum"—a 140-page dictionary culled from Orderic's vocabulary. Chibnall provides Orderic with a short biography, discusses the background of his cloister, analyzes his sources and methods of composition, and reviews the history of his manuscript and of scholarly interest in his achievements.

Orderic was born in England (February 16, 1075) to a French father and an English mother. He lived there until the age of ten when he was sent to Normandy to the monastery of Saint Evroul where he spent the remaining fifty-six years of his life. He gave fourteen years (1123–1137) to the compilation of his *History*, and it reveals him to have been an enthusiastic witness to the age of Norman greatness. Chibnall claims that "his work belongs to the heroic tradition . . . the oral tradition of epic song, which was to be continued later in the written vernacular epics and in the sagas" (p. 77). His view is not circumscribed by a narrow definition of churchly concerns or a rigid tradition of Latin historiography. He wrote in the tradition of those who hoped to "profit future generations" (Prologue, p. 131), and Chibnall has served him extraordinarily well in making the fruits of his labors easily available to us and our descendants.

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MARTIN WEINBAUM, editor. *The London Eyre of 1276*. (London Record Society Publications, number 12.) London: The Society. 1976. Pp. xli, 189.

This volume is a sequel to the *London Eyre of 1244*, in which Martin Weinbaum collaborated with the late Helena Chew. The format has been changed in that the text of the Eyre roll now is given only in the English translation. It would have been helpful for the general reader if a similar translation had been made of the portions of the Estreat roll and Pipe roll pertinent to recording the revenue from this eyre, which are published in this volume. The remarkable legal *notae* by a later hand are well left in Latin as they will interest only specialists. The English text is well done and is accompanied by a good index, though unfortunately it is not keyed to the introduction.

The introduction does more than illustrate the contents of the volume. It delineates the differences between the London eyres, which were essentially crown plea sessions, and other thirteenth-century eyres. Most of the difficult terms are explained, although a few such as *nihil sciunt* will confuse beginners. This volume should be ideal for introducing undergraduates to primary sources; they would have to be supplied, however, with additional information about the governance of London and the relationship of the wards to the jury system.

Because of the infrequency of eyres in London, the volume contains material from 1251 to 1276. A number of entries relate to the barons' wars, but the run-of-the-mill criminal and civil pleas provide interesting materials for understanding law and society in thirteenth-century London. Entries include an abortion case, number 187; arrangements for old age that were not met, number 505; and a boy ward abducted, number 511. The edition for the most part is admirably clear, although entry 14 reveals some of the complexities that face an editor. The item is a straightforward case of misadventure concerning a lady who fell into a vessel full of mash and scalded herself to death. The puzzle is why it should begin by noting that the lands of Arnold de Geraudon and Thomas Sporon are in the hands of one Alice. The editor identifies the men as "chamberlains," who functioned as coroners in the city of London. Arnold and Thomas may have been distrained for defects in their office, but how did Alice get the land? The publication of excellent record society volumes like this one will be an aid in the solution of such questions about the administration of the law.

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BERYL ROWLAND, editor and translator. *Medieval Woman's Guide to Health: The First English Gynecological Handbook*. Kent, Ohio: Kent State University Press. 1981. Pp. xvii, 192. \$17.50.

Medieval Woman's Guide to Health presents an edition and translation of a Middle English gynecological treatise, most of which is devoted to disorders of the female reproductive system. Much of the content and organization reflect the traditions of Latin medical learning; the editor, Beryl Rowland, notes parallels with the Latin writings of Gilbertus Anglicus and Arnald of Villanova as well as with Trotula. The treatments recommended consist chiefly of numerous herbal recipes; a notable feature, however, is a detailed and illustrated set of directions for midwives on the manipulation of malpresentation at birth, material that the editor believes may derive ultimately from Soranus.

Rowland's work is directed toward a general audience. The edition is based on a single manuscript (British Library, Sloane 2463), scholarly apparatus is held to a minimum, and the introduction includes a broad survey of what is known and surmised about the role of women in ancient and medieval medicine. Even in a simplified edition of this kind, somewhat fuller discussion of related manuscripts and more explanatory annotation of the text than Rowland provides would be useful. For example, to choose only one occasion on which fuller annotation would help, the reader is given no hint that "Lilie seyth" (p. 102) is a reference to the *Lilium medicinae* of Bernard de Gordon. Nor is the translation always accurate: for instance, Rowland renders "the brayne of woman is more myhgt than her herte" as "a woman's brain is larger than her heart" (pp. 86–87), thus apparently missing the allusion to the Galenic theory of complexions or temperaments. The meaning is surely that in women, who are always of colder complexion than men, the brain, a cold organ, predominates complexionally over the heart, a hot organ.

Despite the title given to the present work, the extent to which treatises such as this were actually owned and used by women healers or reflect the experience of medieval women is far from clear. In Sloane 2463, for example, the copy of the work edited by Rowland is found in company with a vernacular treatise on anatomy (replete with specific references to major works of Galen and to leading Latin surgical and anatomical writers of the thirteenth and fourteenth centuries) and others on surgery and pharmacology. Although written in different early fifteenth-century hands, the various treatises are sufficiently similar in decoration, size, and layout to suggest at least the possibility that they may be products of the same workshop, either originally commissioned as a collection or assembled early in their history. One wishes that Rowland had addressed the questions raised by the physical resemblances among the works in this manuscript. But whatever the sex, occupation, or education of the first owner of the copy in Sloane 2463 and

whatever the uses to which the work was put, the gynecological treatise is an important example of late medieval vernacular technical prose, and one welcomes its appearance in print.

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COLIN RICHMOND. *John Hopton: A Fifteenth-Century Suffolk Gentleman*. New York: Cambridge University Press. 1981. Pp. xvii, 267. \$37.50.

In a small way this book is a delight. It is an attempt to lead us into and through the interstices of rural and village England in the fifteenth century by means of a semibiographical examination of an East Anglian squire, John Hopton. It has some of the sensitivity for kin, social networks, and world view that we find in Alan Macfarlane's *The Family Life of Ralph Josselin*, some of the eye for traditional life and its mores we encounter in Ronald Blythe's *Akenfield*.

John Hopton, who died in 1478, is an attractive if shadowy figure, largely because of his retiring disposition, the absence of a last will and testament, and the general nature of fifteenth-century documentation. He flirted briefly with the great Neville connection in his youth and with the duke of Suffolk's retinue in his maturity, but he never made any entangling or long-lived alliance with the magnates. Most of his life was one of prosperous and successful restraint, almost one of public self-abnegation. He served as an elector and was a member of a few royal commissions, but he preferred to pay a fine rather than be knighted. He knew the Pastons and probably would have drawn a nod of recognition from the duke of Norfolk, but he was content not to become embroiled in the political turmoil that so dramatically and frequently wracked his region in his lifetime.

Colin Richmond concentrates on three facets of the man: the accumulation of his landed estates and their administration and value; his family; and his network of "acquaintances, neighbours and friends." The work is a microcosmic study based on meticulous use of the Suffolk Record Office and the wills of the Norwich Consistory Court and is aided by a number of B.A. and M.A. dissertations at Keele supervised by Richmond himself. The value of the book, apart from its powerful human qualities, lies in its detailed analysis of comfortable but unspectacular existence. Hopton generally did well in life, but some children died young, some lands were poor investments, and a few lawsuits entangled even this irenic man. We follow the *dramatis personae* brought into Hopton's world by his two marriages (and the second to a strong Thomasin, née Barrington): children, grandchildren, stepchildren, sons-and daughters-in-law. Through these and through

his council, his commissions, and his property dealings, we touch an open-ended world of associates, who ranged in rank from king's judges and the solicitor general to obscure gentry and minor parochial clerics and teachers.

The book needs a tabular presentation of manorial incomes and better genealogical charts, but its warmth surmounts these minor drawbacks with ease. Though the people flash by quickly and laconically, there is much pleasure in studying a quiet man, one in whom "the itch of acquisition" barely burned: "No wonder politics meant nothing to him, and he meant nothing to politicians: if he did not want anything, how could they use him?" (p. 259).

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ELIZABETH M. HALLAM. *Capetian France, 987-1328*. New York: Longman. 1980. Pp. xiii, 366. \$45.00.

It has been forty years since Robert Fawtier completed his *Les Capétiens et la France* (translated into English as *The Capetian Kings of France*). During those decades, that book—despite certain problematical features, among them the author's nationalistic bias—has been regarded as the standard general work on Capetian France. Fawtier's study is now, however, quite dated. In the book under review, Elizabeth M. Hallam has sought to incorporate the many important findings of more recent scholarship into a "fresh account of the period" (p. xii). The aim is laudable; the product, uneven and disappointing.

In fairness, the task undertaken was extremely demanding, and Hallam has indeed read and attempted to synthesize a great deal of material. After two chapters on early French society and politics, she discusses the Capetians and their kingdom in four chapters, the chronological breaks—1108, 1226, and 1270—corresponding roughly to the changing sphere or degree of the exercise of royal power. Within these chapters she follows a combination of chronological and topical order. It should be said to her credit that her organization is in many respects better suited to the needs of the nonspecialist than is Fawtier's strictly thematic presentation. A more nearly authentic and less schematized or deterministic view emerges from her approach.

The negative remarks to be made about the book are, however, very serious. The most obvious substantive gap is perhaps the lack of awareness of the articles by Jan Dhondt that have completely revised earlier views regarding the accession of Henry I; Hallam's presentation of those events is thus fifteen years out of date. Next in importance come the numerous errors of fact in the book—I counted over seventy. Most of these are minor, but their

cumulative effect is unsettling. One finds, to cite an extreme example, the statement (p. 175) that the kings buried at Saint-Denis included Charles Martel and Hugh the Great (who were not kings), the "Carolingian" Odo (who was a Robertine), and Robert I (who was buried at Sens). Discussions of particular topics (for example, the *reditus* [p. 177] or the reversion of appanages [pp. 250–51, 258, 299]) distort the secondary accounts on which they are based and reflect no familiarity at all with the relevant sources. The overall effect is of work hurriedly and often carelessly done.

The book was poorly edited. The writing is choppy, with many dangling modifiers and a few grammatical mistakes. One is puzzled by the repeated statement (pp. 273, 326) that the authority of the last Capetians could be challenged "only with impunity."

It is always profitable to read someone else's overview. Beyond that level, this book should be cautiously used. Both graduate students and specialists should consult it in their first stages of research on the topics that it includes, but they will probably not refer to it thereafter.

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MARINA WARNER, *Joan of Arc: The Image of Female Heroism*. New York: Alfred A. Knopf. 1981. Pp. xxvi, 349. \$19.95.

It is surprising that the outpouring of scholarship devoted to women's history has not produced a full-length feminist interpretation of Joan. That Marina Warner, following her admirable analysis of the cult of the Virgin Mary in *Alone Among Her Sex*, turned to an avowedly feminist study of Joan was cause for hope. Although I must report that Joan still awaits her feminist biographer, Warner's book does move us in that direction. Combining a rich compendium of original observations about Joan's life with a survey of how succeeding centuries have created their contrasting images of Joan, it yet contains serious flaws as historical writing.

The book of course does not offer us new material (there probably will be no new sources until the transcript of Joan's early examination at Poitiers is located) but rather capitalizes on Warner's shrewd reflections on familiar topics: that the Maid's life, our very model of female heroism, was in fact throughout "a tribute to the male sphere of action"; that her popularity since her death rests on her martyrdom rather than on her life; that our "necrophiliac culture" demands that, if she is to be our heroine, she, like Christ and indeed all Western heroes, must die an early and a sacrificial death.

Observations such as these can stimulate further analysis of the sources, but the problem for the

historian comes precisely over Warner's failure to make good on her two chief intentions: "to restore [Joan] to her context, to create a foreground of the religious beliefs and political struggles that made her activities acceptable and intelligible" (p. 8) and "to see how Joan fitted into an intellectual and emotional tradition of thought concerning women" (p. 10). The two failures interrelate, because the chief historical tradition (heresy) and the chief female tradition (mysticism) into which Joan can be fitted overlap. Despite the fact that Joan's voices are central to any interpretation of her life, Warner rejects outright the possibility that Joan was influenced by the tradition of female mysticism, although she allows that it had made a remarkable impression on her times. The author's view of medieval mysticism, in fact, is so narrow as to be arbitrary. Referring to "millionarist [*sic*] doom . . . the torments of the damned [and] the other commonplaces of medieval mystical experience" (p. 89), Warner insists that mystics were entirely otherworldly. For example, she accuses Hildegard of Bingen and Mechtilde of Magdeburg (whom she mistakenly places in "the early middle ages" [p. 78]) of not prophesying about the problems of this world as Joan did, despite the fact that she twice contradicts this judgment about Hildegard (pp. 86, 230) and could have done so in regard to Mechtilde.

On the subject of heresy, Warner is even further off base. Holding that the ecclesiastical institution of Joan's time "permitted people within it to struggle for change without their seeming to aim at revolution or subversion" (p. 69), she repeatedly implies that the fifteenth century was a time favorable to independent religious action. How, then, are we to explain the persecution of the Hussites or the Lollards? Or Joan's condemnation for heresy, for that matter? Warner's great strength, her ability to re-imagine a scene, works brilliantly in her description of how Joan's very thoughts changed under the pressure of relentless questioning by her judges. But having thus demonstrated how the inquisition created heresy where none indeed had been, Warner does not draw the conclusions necessary to link this meaning-laden scene with the value structures of Joan's time. And she scarcely pauses to look at Joan as unwitting challenger of ecclesiastical authority, nor does she place her in the context of other nonconformists. Repeatedly raising issues that she does not fully deal with, she leaves an impression of inconclusiveness as a writer of history.

Warner's scattered statements about women's place in European history during and since Joan's time are so perceptive that one wishes she had placed Joan firmly in the context of actual heroic women rather than stressing her uniqueness. She is correct that Joan's story, usually "told within the available lexicon of female types" will not fit the

mold, but the problem lies in our image of what female autonomy may be, not in Joan's supposed "uniqueness."

The many illustrations and the intelligent discussions that accompany them are a rich resource, but the book badly needs a bibliography. Warner's study will stimulate new questions about Joan's life and its historiography, but it remains only notes toward a portrait of Joan as female hero, rather than the re-interpretation that Joan's story deserves.

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NANCY G. SIRAI. *Taddeo Alderotti and His Pupils: Two Generations of Italian Medical Learning*. Princeton: Princeton University Press. 1981. Pp. xxiii, 461. \$32.00.

Medieval science and medicine are all too frequently characterized as dry, academic subjects that had little to do with life and its day-to-day activities. The standard picture that is usually painted of the medieval physician—at least since the days of Vesalius—is that of a book-bound pedant who seldom glanced up from the pages of Galen to take note of the findings of his barber-surgeon assistants. Nancy G. Siraisi's study, *Taddeo Alderotti and His Pupils*, should once and for all put this standard characterization of the medieval physician to rest. Her detailed and lively discussion of the life and works of Taddeo, one of Bologna's most illustrious doctors, and the men who studied under him—Gentile da Cingoli, Bartolomeo da Varignana, Guglielmo de'Corvi da Brescia, Dino del Garbo, Pietro Torrigiano de'Torrigiani, and Mondino de'Liuzzi—demonstrates very clearly how active the study and practice of medicine were in the thirteenth and early fourteenth centuries.

Since medieval medicine found its primary professional focus in the university, Siraisi quite naturally begins her study of Taddeo and his circle with the academic setting (chap. 1). Brief biographies of the entire group then follow (chap. 2) along with a description of their contributions to the world of learning (chap. 3). The remainder of the book is devoted to an in-depth analysis of the activities of the seven key figures as learned physicians, touching on their impact on the medical curriculum (chap. 4) and its content (chap. 5), the relationship between medicine and philosophy (chap. 6), the range of questions raised by medieval physicians (chap. 8), an analysis of the special problem of the relationship between thought and sense (chap. 7), and finally the practice of medicine itself (chap. 9). Two long appendixes—a list of medical *quaestiones* discussed by Taddeo and his pupils and a register of

known works by Hippocrates and Galen—round out the study.

The characters in Siraisi's book emerge as intellectually lively scholars who took the time to know their patients (mostly men of means) and their problems. As academics, they participated in the intellectual life that surrounded them in early Renaissance Italy. As physicians, they fit comfortably with the empiricism that slowly crept into medicine from the medieval period on. Some of their achievements were noteworthy, with the best-known case in point being Mondino's influential and widely used *Anatomia*; others were no more than typical of the times. As a group they leave little doubt that around 1300 the Western world was slowly but deliberately developing attitudes toward science and medicine that would eventually lead to profound changes in its way of life. Siraisi's book is an important contribution toward understanding early modern science and medicine in its broadest context and deserves to be read and studied by all historians who are interested in the early modern period.

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MODERN EUROPE

OWEN CHADWICK. *The Popes and European Revolution*. (Oxford History of the Christian Church.) New York: Clarendon Press of Oxford University Press. 1981. Pp. ix, 646. \$84.00.

Owen Chadwick has created a highly nuanced, complex mosaic of eighteenth-century Catholicism, focused on the papacy and its relationship to the Catholic peoples of Europe and America during the eighteenth and early nineteenth centuries. The book's greatest strength is its topical description of the church of the *ancien régime*. The author asserts that the Counter Reformation continued, although with diminishing vigor, throughout the period. He evinces sympathetic tact toward the primitive, superstitious religion that flourished among common people, the archaic exemptions and privileges of the clergy, and the declining institution of monasticism. Chadwick is at his best on the papacy. He raises our estimation of the character of most of the popes from Clement XI to Leo XII while pointing out their political ineptitude or impotence. Clement XI (1700–21), for example, was "the saintliest," as well as "the worst pope of the century." Able, courageous, and devout, he consistently backed the losing side in the War of the Spanish Succession; he condemned Quesnel's *Moral Reflections* with a bull (*Unigenitus*) that launched the worst religious con-

trovery of the eighteenth century; he suppressed the Jesuits' experiment with Chinese rites, killing the infant Chinese church.

The book's second part, devoted to reform and revolution, allows us to see the popes' incomprehension of social and cultural changes affecting the Catholic church that laymen understood. Concerning the controversy between Joseph II and Pius VI over the Austrian policy of religious toleration, Chadwick remarks: "the Emperor cannot be denied a more sensitive understanding [than the Pope] of the needs of justice in contemporary society" (p. 435). There is only a thin narrative thread through this period. The author has described "what Catholicism was like before the deluge and what it was like after" (p. v). The reader is referred to other histories to find out how the deluge took place and why.

Chadwick emphasizes the static elements of eighteenth-century Catholicism: popular religion and ecclesiastical institutions. He pays more attention to political than to spiritual or intellectual forces of modern reform. There is little description, for example, of the remarkable diffusion of Jansenist and other enlightened literature, especially after 1760. The author writes off *Nouvelles Ecclésiastiques* as "the bitter journal of French Jansenists" (p. 402). His further remark that the journal (1728–1803, not 1790) was "often deplorably edited" (p. 627) is perhaps unfair. He overlooks the journal's widespread distribution, its warm reception by Maria Theresa, Joseph II, and other persons in authority, and its network of correspondents, which kept Catholic reformers in touch with one another.

The author occasionally has difficulty with the elusive phenomenon of Jansenism. He correctly distinguishes French Jansenists, preoccupied with predestination and other theological issues, from the more pragmatic Italians. His treatment of Austrian Jansenism, however, is less satisfactory. He identifies erroneously Gerhard van Swieten as "the centre of the Jansenist group" in Vienna (p. 394). Peter Hersche and Erna Lesky, both listed in the author's bibliography (p. 628), have shown that van Swieten was not a Jansenist. There is evidence only that he was exposed to, rather than "affected early in life by Jansenist ideals" (p. 413). He often opposed Jansenist activity in Vienna.

Chadwick's book is an extended essay, intended for the "lay" reader who already knows the chronology and the main trends of the period. Footnotes are few. The bibliography consists of an impressive list of works consulted. There are perhaps more lapses in editing than might be expected from a prestigious university press, for example, an incoherent paragraph (p. 447) on papal reaction to the French Civil Constitution of the Clergy. The result of lifelong study of the history of the Christian

church, this book deserves careful and reflective reading.

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ARNO J. MAYER. *The Persistence of the Old Regime: Europe to the Great War*. New York: Pantheon. 1981. Pp. xi, 367. \$16.95.

Arno J. Mayer argues here that in the period 1848–1914 it was Europe's postfeudal nobility, not industrial and finance capitalism, that dominated civil society as well as foreign policy. This is a curiously conservative argument for someone with Mayer's credentials, yet he implies that these credentials actually give weight to this argument. "I conceive of this book as a Marxist history from the top down," he writes, "with the focus on the upper rather than the lower classes" (p. x). It is indeed "history from the top down"; whether or not it is "Marxist history" is debatable. In many ways it is an updated version of Joseph Schumpeter's rebuttal of Rudolf Hilferding. The two main premises that pervade Mayer's book are: (1) "the forces of the old order were still sufficiently willful and powerful to resist and slow down the course of history" even if they had to start a general war, and (2) "Europe's old order was thoroughly pre-industrial and prebourgeois" (p. 4). Mayer sets out to counterbalance the tendency of both liberal and Marxist historians "to neglect or underplay, and to disvalue, the endurance of old forces and ideas and their cunning genius for assimilating, delaying, neutralizing, and subduing capitalist modernization, even including industrialization" (p. 4).

Undoubtedly elements of the old order persisted into the twentieth century, and Mayer performs a valuable service in highlighting them. But two large questions arise: How widespread was this persistence in different parts of Europe? What aspects of European society can legitimately be labeled "pre-industrial" and "prebourgeois"?

Mayer's thesis is most persuasive regarding the three great empires of Central and Eastern Europe, parts of Spain, and southern Italy; it is less convincing for Great Britain and not convincing at all for France, Switzerland, the Low Countries, Denmark, Norway, and northern Italy. Even within Wilhelmine Germany the forces of the old order were far stronger in East Elbian Prussia than in the Ruhr, although the postfeudal nobility was certainly dominant in the governing classes in Berlin. In Russia the degree of political, economic, and social domination by the nobility was more problematical; Russian nobles owned a far smaller proportion of the land than their German or British counterparts, and

Mayer dutifully stresses their role in state service, but even there he notes that the majority were ennobled career civil servants of whom only the more influential ones "continued to have links with the land" (p. 120). As for France, Mayer consistently admits that it was an exception to most of his generalizations. Still there is much to be said for the persistence of an Old Regime world view in politics, culture, and social relations in the Hohenzollern, Habsburg, and Romanov empires. Mayer's argument breaks down, however, when he harps on the preindustrial and prebourgeois basis of pre-1914 civil society, especially in Britain and Germany.

Mayer believes he must make economic relations the basis of everything else, but he is least persuasive on this point. Is it really necessary to insist that "cosmopolitan merchants and bankers, along with local manufacturers, continued to carry more weight than the owners and managers of big industry and corporate banking" in order to convince us that landed aristocrats still dominated civil society? It may be that landed aristocrats in Central and Eastern Europe—and to some extent in Britain—retained and regained more power and influence *despite* the changing economic balance because they were able to parlay their social and cultural prestige with political skill and superior connections.

Mayer offers a refreshing alternative to the usual emphasis on modernism in high culture. It is certainly true that the dominant styles in the arts were tradition bound and that until 1914 avant-gardes were even more marginal than republicanism and finance capitalism. But is a traditional, "classical" style supportive only of the old aristocratic order? (One thinks of the French Revolution.) Mayer rightly stresses the role of elitist secondary schools in "the reproduction of the world-view and learning of the old notables" (p. 253). But here as elsewhere he weakens his argument by using "notables," "aristocrats," "ruling class," and "upper class" interchangeably.

Despite some strains in the argument, particularly in the frequent "Admittedly . . . Even so" passages, this book is an important corrective to the view of bourgeois capitalism as "hegemonic" before 1914. It is surely more plausible to blame the outbreak of the Great War on an ultraconservative resurgence among soldiers and statesmen than on imperialism as the highest state of capitalism. And Mayer is not the first historian to link this resurgence with the appeal of Social Darwinism and the Nietzschean critique of modernity in "the upper reaches of polity and society" (p. 290). This thoughtful and learned work will provoke a lively controversy and inspire a flock of new studies designed to reinforce or challenge the latest "Mayer thesis."

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PAOLO BRUNDU OLLA. *L'equilibrio difficile: Gran Bretagna, Italia e Francia nel Mediterraneo, 1930-1937*. (Università di Cagliari, Pubblicazioni della Facoltà di Scienze Politiche.) Milan: Dott. A. Giuffrè Editore. 1980. Pp. 245. L. 10,000.

This solid study, based on careful research in unpublished British and Italian documents as well as published materials, describes unsuccessful French efforts from 1929 to 1936 to align Italy and Britain closely with France in a Mediterranean agreement or "Mediterranean Locarno" that would help prevent German and Italian expansion. Paola Brundu Olla also discusses the formation of the Anglo-Italian Gentlemen's Agreement of January 2, 1937. The great merit of this study lies in its description of the disarray in the relations of the three states. Here is another chapter in the record of British and French failures of judgment and will, mistrust, loss of initiative, unpreparedness, and bungling as German military power mounted and Hitler and Mussolini unfolded their aggressive plans.

Jealous of France's Mediterranean position and with his own ambitions in the area, Mussolini was unwilling to accept the French proposals. The German threat to Austrian independence and Mussolini's search for a free hand to conquer Ethiopia led to the Laval-Mussolini agreements of January 7, 1935, followed by an air convention in May and a military convention in June, but no permanent improvement in relations resulted. Mussolini never forgave France for joining even half-heartedly in sanctions, and he developed a deep antagonism toward the Blum government, which he regarded as a spearhead of communism in Western Europe. He sought the isolation of France and the fall of Blum.

The Anglo-French friendship was also under great strain and the two nations had divergent views on many European questions. Britain felt no insecurity in the Mediterranean prior to 1935 and was unwilling to assume commitments beyond the Locarno agreements of 1925 and the League Covenant. Britain resented the Laval-Mussolini agreements, mistrusted the Blum government, and was determined to prevent formation of an antifascist bloc that could come under communist domination. Neither British nor French leaders cared much for the fate of Ethiopia, but British public opinion and the coming elections forced the British government in 1935 to push for incomplete sanctions against Italy, which France reluctantly joined. With the threat of some mad act by Mussolini, Britain concentrated ships in the Mediterranean, entered into temporary mutual assistance agreements with Yugoslavia, Greece, and Turkey, secured assurances of French collaboration, formed an alliance with Egypt, and resolved to defend its supremacy in the Mediterranean.

Britain, unlike France, underestimated Mussolini's flirtations with Hitler. Britain and France also viewed the Spanish Civil War quite differently. French leaders favored the Spanish Popular Front government and were greatly alarmed at German and Italian intervention, especially Italian activity in the Balearic Islands. British leaders favored a Franco success—providing British interests in Spain were respected—and actually saw that Italy could be useful in preventing too strong a German position.

The Gentlemen's Agreement of 1937 pledged respect for the rights and interests of all Mediterranean states, support for the territorial status quo, and freedom of exit, entry, and transit in the Mediterranean Sea. This agreement, coupled with letters in which Italy promised to respect the status quo in the western Mediterranean and the territorial integrity of Spain, may have restored a measure of Anglo-Italian friendship, but Britain did the yielding with regard to specific issues. The newly formed Rome-Berlin Axis had already begun to pay dividends.

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JOHN TERRAINE. *To Win a War: 1918, The Year of Victory*. Garden City, N.Y.: Doubleday. 1981. Pp. xvi, 268. \$14.95.

As the most prolific writer on the First World War during the last two decades, John Terraine has waged a vigorous campaign against what he believes are the misconceptions many of his countrymen have about the role and performance of the British empire's forces on the Western front. Through articles in *History Today*, numerous books, reviews of books in the British press, and as the scriptwriter for "The Great War" television series, he has attempted to counter the critical assessments of Sir Douglas Haig's strategy and tactics. His best-known work remains his biography of Haig. Although *Douglas Haig: The Educated Soldier* (1963) has been called "an essay in vindication" by A. J. P. Taylor, Terraine did not believe that he had given the commander-in-chief of the British Expeditionary Force all the credit that was his due. When he published his *The Western Front, 1914–1918* (1964), he lamented that he had not "said enough about the astonishing sequence of victories which he [Haig] won in 1918" (p. 179).

This work, which focuses on "100 days of practically unceasing successful attacks unparalleled in British military history" (p. 84), is apparently his effort to set the record straight once and for all. He argues that the Allied attacks (with British arms playing the leading role) that began with the battle

of Amiens finished off the Central Powers although "generations of clever men have failed to notice this" (p. 102). Terraine does not pull his punches in dismissing the arguments of Haig's critics, most notably David Lloyd George, who, horrified by the gigantic losses of the British army on the Western front, favored an indirect or "Eastern" strategy to defeat Germany. To Terraine (and Haig) the only theater that mattered was in France and Flanders. Lloyd George's schemes to concentrate on Germany's allies ("knocking away the props") were "rub-bish" (p. 20). Germany, he argues, propped up its allies, not the reverse. Carrying this line of reasoning to an extreme, he asserts that both Bulgaria and Turkey were actually defeated in France.

As always, there are flashes of insight in Terraine's examination of the war. The 1918 tank, he convincingly demonstrates, was not a "war-winning weapon" (p. 98). The nature of the unity of command, achieved belatedly in 1918, is often misunderstood. Ferdinand Foch by necessity was more the Allied persuader-in-chief than commander-in-chief. Haig, rather than being Foch's instrument, was his partner in planning and executing the victorious battles. Enlightening comments are also made on America's role in the war.

Terraine in this and other works demonstrates that Haig was neither a stupid general nor a callous butcher. His achievements in 1918 reflect his considerable development as a strategist and tactician; his courage and tenacity have never been in doubt. One is struck by the sobering thought that if Lloyd George had not been restrained by the possible political repercussions he might have replaced Haig before the great battles of 1918 with Claud Jacob, the commander of the II Corps, hardly a household name either then or now. Haig, however, had some truly amazing blind spots. Until the last shot was fired he continued to believe that the cavalry had a leading role to play on the Western front. To his credit, Terraine does mention this but he clearly does not give this matter the same attention that Haig does in unpublished entries in his diary. Haig, in fact, gave far more personal attention to the horse soldier than to the tank.

Not everyone will be convinced by Terraine's staunch defense of Haig and the "Western" school of thought. But Terraine continues to make a significant contribution to the literature of the war by being the primary catalyst for the rethinking of British strategy and tactics. Would that he could show as much understanding of the way the war looked in 1918 to the British and Dominion civilian leaders. This perspective might have been gained if Terraine had mined the rich lode of private and official papers in Britain, most of which have been available to the scholar since the mid-1960s.

This work may well give the scholar a new view-

point, and the clear and dramatic writing style is certain to delight the casual student of the war.

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JAMES D. WILKINSON. *The Intellectual Resistance in Europe*. Cambridge: Harvard University Press. 1981. Pp. x, 358. \$20.00.

"The men of the Resistance," declared *Combat* in 1943, "will be the builders of a new Europe tomorrow" (p. 174). James D. Wilkinson's *The Intellectual Resistance in Europe* begins with this bit of hubris, asking what precise effect the World War II Resistance experience had on hopes and dreams for the postwar world formulated by the intellectual elite of the European underground, and how those dreams worked out.

Wilkinson's focus is on the French, German, and Italian intellectuals who attempted to carry the ideals and enthusiasms of the war into the troubled peace that followed. His subjects—Jean-Paul Sartre, Albert Camus, Simone de Beauvoir, Ignazio Silone, Carlo Levi, and Heinrich Böll are perhaps the best known among them—were generally men and women of the liberal left, believers in freedom, social justice, and a broadly conceived humanistic "renewal" for Europe after the war. To achieve their ends, these engaged intellectuals tended to depend not on the postwar governments or revived political parties but on the moral authority, commitment to high ideals, and classless capacity for fellow work they had found in the Resistance movements. Theirs was "politics reduced to elemental choices for and against" (p. 62)—a simple code that could not long survive the restoration of politics as usual, the complexities of peacetime social conflicts, and the polarizing surge of the Cold War.

To support this interpretation Wilkinson offers a satisfying range of evidence. He has read widely in the wartime and postwar literature, journalism, and personal memoirs of the three nations under consideration. He has supplemented this material with personal interviews with several dozen of the Resistance intellectuals themselves.

Wilkinson's thesis best fits the French intellectuals around whose archetypal experience it would appear to have been designed. His interpretation holds less well for the Germans, whose postwar passion for truth and freedom looks more like a sharp contrast with the "camouflaged" press, spiritual "inner emigration," and general Aesopian language of the Hitler years than a continuation of any Resistance militance. In the broadest sense, however, the book does analyze convincingly the ideological pilgrimage of a generation of European writers and thinkers from spiritual isolation during the prewar years through the emotional engagement of

the war to failure and disillusionment in the later forties. Indeed, the generational dimension of the World War II Resistance, repeatedly mentioned but never developed, might usefully have been explored further. The literature of the "historical trauma" as a factor in the formation of generational consciousness grows apace—Robert Wohl's *Generation of 1914* (1979) comes immediately to mind—and the Resistance experience of these younger European intellectuals who clearly "perceived themselves as belonging to a sharply defined generation" (p. viii) would seem to be a case study made for generational analysis.

Such cavils aside, however, Wilkinson's *The Intellectual Resistance in Europe* is a sensitively written piece of intellectual history and a valuable addition to the burgeoning literature of the European *maquis* and partisan movements during World War II.

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MURIEL ST. CLARE BYRNE, editor. *The Lisle Letters*. In six volumes. Chicago: University of Chicago Press. 1981. Pp. xxviii, 713; xviii, 705; xv, 633; xv, 546; xxi, 770; ix, 472. \$250.00 the set.

Arthur Plantagenet, Viscount Lisle, was an illegitimate son of Edward IV. Born about 1462, he lived in relative obscurity until 1533, when Henry VIII appointed him Lord Deputy of Calais. In 1540 he was recalled and subsequently imprisoned in the Tower. He was fully pardoned in 1542 but died (of "immoderate joy," according to John Foxe) before he could enjoy his freedom.

At the time of Lisle's fall his papers were confiscated, evidently in the hope that they would provide evidence of treason or heresy. For this unusual reason they have survived virtually intact. They are unusual in scope as well; only someone living outside England but involved in both official and private business at home would have generated such a bulk of correspondence. The letters printed here—nearly 1,700 of them—are but a selection, though a large selection, from seventeen volumes of manuscripts preserved among the State Papers in the Public Record Office.

This material is not wholly new to historians, for the *Letters and Papers of Henry VIII* contain quite full and generally accurate summaries of most of the letters. But calendars cannot convey the flavor of literary styles or the intimacy of personal exchanges, and it is only when the Lisle letters are read in their own sequence, not interspersed with other government documents, that the tragedy of the deputy's situation becomes manifest. There is thus real reason to be grateful to Muriel St. Clare Byrne and to her publishers for this splendid edition. The project, clearly a labor of love, has been under way for

half a century (T. S. Eliot was among its early supporters); the volumes form an eighty-fifth birthday tribute to their editor.

So rich are these letters, so varied their interest, that a review can do little more than call attention to some outstanding points. There is, first, the character of the correspondents. Lord Lisle himself, usually pictured as a dull, aging aristocrat quite incapable of controlling affairs in Calais or even of managing his own finances, now emerges as an active, conscientious administrator. If affairs at Calais were not well handled, it was because officials in London (mainly Thomas Cromwell and the king) did not respond to Lisle's repeated pleas for policy decisions and essential funds; if he was unable to pay his bills, it was because he lived in appropriate state and practiced lavish hospitality without adequate allowances from the English government. Lisle's wife, Honor, memorably characterized by Foxe as a dishonorable idolater, hypocrite, and "incomparably evil," was in fact none of those things. She was instead a shrewd politician, a charming hostess, an effective administrator in her own right, a good mother, and an affectionate wife. The letters exchanged between the Lisles, on the few occasions when they were apart, are especially touching: Lady Lisle addresses her husband as "Mine own Sweet heart" and signs herself "her that is more yours than her own," while Lord Lisle closes his response, "I do as much long for [you] as doth the child for his nurse, and that knoweth God" (vol. 5, pp. 282, 284, 314). One can understand why Lady Lisle lost her mental stability when Lisle was imprisoned. While a number of children figure in the correspondence—Lady Lisle had seven and Lord Lisle three, both by previous marriages—the third character to emerge as a fully rounded personality is John Hussee, the Lisles' faithful agent in London, always busy promoting their lawsuits, handling business at court, badgering Cromwell, shopping for clothes and provisions, and writing perceptive accounts of activities in Parliament and at court.

Materials for social history abound. Those interested in costume and clothing will revel in the inventory of Lady Lisle's wardrobe made at the time of Lisle's fall and in references to her lace nightcaps, decorated nightgowns, sleeves embellished by Henry VIII's court embroiderers with hundreds of pearls, saddle of Lucca velvet, and jewelry restyled by Parisian artisans. There are also bills for the twenty-nine pairs of assorted shoes and boots ordered by Lord Lisle in 1535, for "study gowns" made for a son at Lincoln's Inn, and for elaborate apparel provided for a daughter who was a maid of honor to Jane Seymour. Food and drink loom large, with references to French wine sent as a bribe to officials in London, barrels of sprats to be consumed in Lent, and such delicacies as venison pasties, boar's

head, and baked crane. Historians of medicine will find the predictable number of purges, but also fascinating information about Lady Lisle's false pregnancy and Lord Lisle's letter commending a skilled French physician while damning an unlearned barber-surgeon. There is material about the weather; about the education of children in Paris and in the homes of French aristocrats; and even about pet animals like Purquoy, a specially appealing dog sent reluctantly by Lady Lisle to the English court, the monkey that Hussee advised the Lisles not to give Anne Boleyn (she detested monkeys!), and the pet bird that survived shipwreck only to be devoured by a cat.

The religious changes associated with the earliest stages of the Reformation find their place here as well. Following the dissolution of the monasteries—and after much trouble with bureaucracy in the Court of Augmentations—Lisle acquired a priory in Devon and a friary in Calais, although they did little to ease his imminent bankruptcy. Lady Lisle was admonished by Hussee not to appear too religious, for it was thought popish, and to abandon certain of her observances; she tried to obtain a French or English translation of the Bible, only to find that conservatives had blocked publication. The bitter strife between reformers and papists in Calais, largely responsible for Lisle's fall, has been studied previously, but it is now better documented than ever before. In short, there is hardly any aspect of early Tudor life that is not mirrored in these pages.

Both Byrne's editorial practices and her new interpretations of men and motives merit attention. At first approach the editorial methods seem inconsistent and unscholarly. The text of the letters, for instance, is generally modernized, but closing paragraphs are not, and a handful of letters are transcribed with the original orthography and (less often) original punctuation. Letters written in French are translated into English—not modern English, which would contrast sharply with the style of other correspondents, but the sort of English written in the 1530s. Not all the letters in the Lisle manuscripts are printed, but some related bits from other sources have been included. Organization is generally chronological but not strictly so: special chapters are devoted to the care and education of children and to the Calais Commission and the Botolph conspiracy. A significant amount of space is given over to introductions, conclusions, and editorial comments of all sorts, and the last two chapters deal entirely with the period after the letters cease. The surprising thing is that, in Byrne's hands, these procedures work. The blend of her own writing and the correspondence itself brings the letters to life in such a way that the volumes can be read with delight by persons who would never tackle a more traditional scholarly edition, yet it provides serious historians

with complete texts that have never been available in print.

Most of the new interpretations succeed as well. One might have expected a rehabilitation of the Lises, but not one so fully documented and manifestly justified. Similarly, one is not surprised to see Cromwell or Henry or the Court of Augmentations harshly treated; what does surprise is how convincing, in this context at least, the views are. Byrne argues that Calais was central in the military and religious crisis of 1540 (possibly an exaggeration), and she provides a fascinating reconstruction of the interrelation between Lisle's fall and Cromwell's (probably correct). Among the historians with whom she takes issue are R. B. Merriman, W. C. Richardson, and A. J. Slavin. She speaks respectfully of G. R. Elton but clearly rejects a number of his judgments. Occasionally time has overtaken her revisions. Little published in the last fifteen years has found its way into the bibliography, and it is no longer correct (for instance) to say that no one has pointed out how near Lisle came to being attainted by Parliament in 1540 (vol. 6, p. 167). Such lapses, however, are minor and understandable.

The Lisle letters deserve to be as well known as the Paston letters. The largest body of personal correspondence from the sixteenth century, they rank with the most interesting personal documents of any era.

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RICHARD M. WUNDERLI. *London Church Courts and Society on the Eve of the Reformation*. (Speculum Anniversary Monographs, number 7.) Cambridge, Mass: Medieval Academy of America. 1981. Pp. xiii, 163. Cloth \$12.50, paper \$5.00.

Court records—both lay and ecclesiastical—survive in quantities that are daunting to the English historian. Yet they are rewarding not only for understanding the evolution of legal practice but also for light they cast upon social conditions. Richard M. Wunderli's *London Church Courts and Society on the Eve of the Reformation* indicates their dual significance. His aim is twofold: first, to describe how ecclesiastical courts in the diocese of London functioned; second, to demonstrate the thesis that "before the English Reformation a profound shift in attitudes of lay people had already taken place" (p. 2). These two goals tend to get in each other's way, because the author presses his thesis a bit too bluntly.

Wunderli's subject is less broad than his title indicates. He does survey the various ecclesiastical courts functioning in London, both those of the bishop of London and the "peculiars" of other religious corporations. He also gives descriptions of

the work of London's church courts, useful to any legal historian. His evidence, however, comes essentially from the bishop's commissary court, because continuous records of its proceedings survive from 1470 to 1516. This evidence shows that Londoners, finding the commissary court inefficient and ineffective by the late 1490s, were turning more to the city courts for justice. No sharp line separated the two jurisdictions, and cases concerning debts, sexual misconduct, and even heresy could come before either tribunal. Complainants found a higher conviction rate and harsher punishment in the lay courts. Wunderli makes clear that procedures of the church courts functioned well only in small and stable communities where everyone knew everyone else's business. The dependence on *mala fama* or "public rumor" for reporting suspects, on compurgation by neighbors as proof, and on threat of excommunication as a sanction limited the commissary court's effectiveness. The anonymity of the lives of the poor made such procedures ineffectual, as did the shamelessness of a large criminal underworld in England's largest city.

The author pictures the drift from church to city courts as "part and parcel of the secularization of society in the late medieval world." At the same time, he recognizes that the shift may indicate the secular government's wish to be "a more active partner within the Christian body to enforce Christian norms" (p. 137). Such a concern by the state may not be so much a sign of secularization as of spiritualization of lay society: perhaps laymen were thinking that enforcement of Christian conduct was too important to be left to the church courts. As Wunderli points out, such an attitude prepared the way for the Reformation Parliament's work.

This book does describe well the work of the London commissary court, 1470–1516, and it documents its decline as more of its business moved to the secular courts. The author is perhaps less convincing in demonstrating increased secularization of London life. Shaping a clear account of the London commissary court out of the raw records, however, is no small accomplishment, and Wunderli deserves credit for completing that goal successfully.

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LELAND H. CARLSON. *Martin Marprelate, Gentleman: Master Job Throckmorton Laid Open in His Colors*. San Marino: Huntington Library. 1981. Pp. xix, 445. \$25.00.

Three months after the defeat of the Spanish Armada, a pseudonymous and inflammatory *Epistle* was published lampooning a ponderous anti-Presbyterian treatise by John Bridges, dean of Salisbury. The first of seven published works by "Martin

Marprelate," a brilliant Puritan satirist, the *Epistle* and its sequel, the *Epitome*, provoked a counterattack by the bishop of Winchester, Thomas Cooper, who in turn was caricatured in subsequent Marprelate tracts. The satirical thrusts at the ecclesiastical hierarchy and the unconcealed hostility toward religious persecution, the oath *ex officio*, the High Commission, and episcopalian polity infuriated government and church leaders, and even caused concern in Puritan circles. With the support of Lord Burghley and Christopher Hatton, Archbishop Whitgift directed an intensive campaign to unmask those responsible for the tracts. More than a score of books and pamphlets were published to refute Martin Marprelate, who himself was caricatured on the stage. The controversy must have been a factor in the government's decision to crush the Presbyterian movement in the early 1590s.

Although a few knowledgeable men, such as Matthew Sutcliffe, dean of Exeter, suspected that the author of some of the Marprelate tracts was Job Throkmorton, a Puritan and an MP for Warwick in 1586, the names of at least twenty-two men have from time to time been proposed as the writer. Most of these can be readily dismissed, but substantial effort has been expended in recent years by Donald McGinn to prove that Martin Marprelate was the Welsh Puritan John Penry. As a result of this meticulously researched book by Leland H. Carlson, the leading American authority on Elizabethan separatists, however, the question of Marprelate's identity has been determined beyond any reasonable doubt. Relying on an intensive analysis of Throkmorton's style, vocabulary, interests, and literary techniques, Carlson convincingly demonstrates that this frustrated idealist and self-assured satirist was indeed the elusive Martin. McGinn's case for Penry is systematically—and harshly—demolished. Unlike Penry, who was averse to personal diatribes and vindictive remarks, Throkmorton, as Carlson shows, willingly resorted to sarcasm and libel; the restrained manner of Penry is in striking contrast to the scurrilous and humorous writing of Throkmorton. Comparing the known works of the latter with the Marprelate tracts, Carlson demonstrates a clear affinity of style and content that is not found in Penry's compositions.

In a tour de force, Carlson not only establishes the authorship of the Marprelate tracts but also argues that Throkmorton's restless pen was responsible for such stinging Puritan critiques as *Master Some Laid Open in His Coulers*, *The State of the Church of Englande*, and even *A Second Admonition to the Parliament* (usually attributed to Thomas Cartwright). In effect, Carlson contends that Throkmorton wrote no less than sixteen separate items (including three parliamentary speeches), a corpus larger than scholars have hitherto suspected. The faults of the book

(redundancy and a few strained or questionable arguments) are insignificant in this masterful study in literary criticism. Carlson has ratified the decision of the grand jury at Warwick in 1590, which indicted Throkmorton for composing the Marprelate tracts. With the corpus of Throkmorton's writings now established, there remains the task of providing a full historical analysis of the man and his works.

RICHARD L. GREAVES
Florida State University

ESTHER S. COPE. *The Life of a Public Man: Edward, First Baron Montagu of Boughton, 1562–1644*. (Memoirs of the American Philosophical Society, number 142.) Philadelphia: The Society. 1981. Pp. xv, 224. \$9.50.

Although Edward, first baron Montagu of Boughton, whose public career is analyzed in this book, lived half of his life during Elizabeth's reign, the nature of the extant evidence has forced his newest biographer, Esther S. Cope, to focus on his Stuart years. Cope's utilization of his diary of the Short Parliament, discovered by her at the Northamptonshire Record Office, in 1969, and many unpublished public and private manuscripts make her work a noteworthy publication. While Montagu's parliamentary career, which he began in 1584 as a representative of Devonshire borough and ended as a member of the House of Lords in the Long Parliament, is of considerable historical importance because of his detailed notes on some of the Stuart sessions, his contribution to the momentous political crises was minimal. In the Parliament of 1628, which enacted the Petition of Right, for example, Montagu's major concerns were the enforcement of religious laws and questions of privilege rather than issues involving the constitutional rights of the king's subjects.

The book's most interesting sections give information about local events in Northamptonshire and sibling relationships in the Montagu family. Forced to live in London until 1602, the year of his aged father's death, Montagu waited impatiently for the opportunity to assume the public responsibilities of a country gentleman. His long-anticipated possession of the Northamptonshire estates resulted in numerous disappointments, as he generally failed in his competition with the Fanes and other aristocrats to dominate local politics. When in 1616 at the age of 54 he finally became the father of a son, his new joy was similarly spoiled, since his desire to provide for that child's future was largely the cause of bitter legal disputes with his younger brothers who had hitherto expected to be his heirs.

Unfortunately, the book is flawed by contradictory statements about Montagu's religious beliefs. In her conclusion Cope identifies him as a Presbyterian, completely disregarding her earlier quotation

from Sir Philip Warwick's seventeenth-century biography of Montagu, which states that although his lordship's severity had caused some to believe he was a Puritan, he had died a "true son of the Church of England" (p. 3). Her evidence for doubting Warwick's contemporary opinion rests largely on Montagu's expressed approval of a few Puritan sermons near the end of his life. That he was uncomfortable with the Arminian episcopate and its altars, carefully observed the Sabbath, appreciated godly works by a variety of Christians (including a Spanish Augustinian), fought for a learned ministry, and insisted on the enforcement of the religious laws is evidence that lends itself more easily to the conclusion that he was a moralistic, conservative church member who disliked Arminian innovations.

Despite the political and religious chaos around him, Montagu remained loyal to his monarch. He died in 1644 under surveillance by a Puritan parliament for having agreed at the age of eighty to serve as a member of the royal commission of array in Northamptonshire. His death spared him the agony of witnessing the royal execution.

RETHA M. WARNICKE
Arizona State University

CLIVE HOLMES. *Seventeenth-Century Lincolnshire*. (History of Lincolnshire, number 7.) London: History of Lincolnshire Committee, for the Society for Lincolnshire History and Archaeology. 1980. Pp. xiv, 279. £9.50.

British historians have recently been much agitated about the validity of one of their favorite concepts: "community." It has even been suggested that the term ought to be abandoned altogether. The truth, of course, is that short of attempting the heroic and studying the whole of humanity, the historian has no choice other than to concentrate on communities of one sort or another—state, church, province, village, or something in between. And, given that people have always been conscious of inhabiting one or more of these communities, the narrowing of focus is legitimate. Happily, in *Seventeenth-Century Lincolnshire* Clive Holmes considers the theoretical difficulties and then escapes from them to provide an impressive addition to the expanding genre of English country histories.

Holmes has already made some telling criticisms of the excessive parochialism and elitism of the "county community" school. In this book he practices what he has preached, skillfully presenting regional politics as a continuing interaction between Lincolnshire and London, between county and kingdom. An introductory group of analytical chapters describes the social and economic base in the towns and villages and then introduces gentry,

lawyers, and clergy as "brokers" between the local communities and the wider political and social universe. Holmes shows that local isolation was eroded in other ways. In an increasingly commercial society, for instance, market towns brought people of different villages into regular contact with one another, and there was the familiarly high level of geographical mobility. An examination of the county's institutions—in this case complicated by the three divisions of Holland, Kesteven, and Lindsey—frequently reminds us of the intrusion of a national political culture into the localities. A series of narrative chapters then traces these forces through the stormy early decades of the century, the Civil War and its aftermath, and, eventually, the Revolution of 1688. Most comparable county histories stop short at 1660, so it is particularly useful to observe the century's continuities as well as the impact of the great eruption in its middle years.

For all its focus on a single county, the book is still a brilliant piece of compression, and the reader will often pine for more detail. Holmes does not quite fulfill his promise of expanding the "county community" to encompass the total society; in the narrative chapters especially he provides something not very different from the gentry-dominated history that he has rightly criticized, except when explosive popular protests against fen drainage bring the common folk into the foreground. The early chapters take us closer to the grass roots than other county studies have done, but they do not illuminate the lower levels of society as vividly as they do the gentry and clergy. Women are also noticeably absent from the discussion. This of course is largely explained by the nature of available sources, but one occasionally wonders if ecclesiastical and other court records could have been exploited even more intensively. Holmes points the way to the grass roots, however, and such reservations do not minimize the value of this contribution to seventeenth-century studies.

DAVID UNDERDOWN
Brown University

J. C. DAVIS. *Utopia and the Ideal Society: A Study of English Utopian Writing, 1516–1700*. New York: Cambridge University Press. 1981. Pp. x, 427. \$49.50.

A primary concern in this exhaustive study is definition. Inescapable in our age of methodological preoccupation, such a concern is especially pertinent to the study of utopias. Utopian thought is not just about ideal societies but about a particular kind of ideal society, and clarifying the distinction between them is one of this book's most valuable contributions. All visions of an ideal society, J. C.

Davis contends, have to face the fundamental problem of creating a society "where social cohesion and the common good are not imperilled by individual appetite." By looking at the way they deal with this basic source of conflict and misery, it is possible to distinguish between utopias and other ideal societies. Davis finds four alternatives to utopia, types "not in practice mutually exclusive, although their premises make them logically inconsistent one with another": the Land of Cockayne, where "there are satisfactions enough to satiate the grossest appetite"; arcadia, where an abundance of satisfactions is balanced by desires moderated or simplified to a "natural" level; the "perfect moral commonwealth," in which the collective problem is solved not by increasing the satisfactions available, but by the citizen's willingness to limit his own appetites (a solution common in English humanist thought); and, finally, the millennium, that terrestrial, imminent cataclysm envisioned by many seventeenth-century sects, as a result of which human problems are solved, as it were, by a *deus ex machina*. By contrast, the utopian accepts the basic problem as it is. He seeks a solution "not by wishing the problem away nor by tampering with the equation." Nor does he assume drastic changes in nature or man. Human nature being essentially recalcitrant and sinful, the utopian solution thus involves discipline, and its institutions tend inevitably to become totalitarian. Hence the paradox of the utopian vision: a radical attempt to escape the nightmare world of injustice and conflict, it ends by denying the autonomy of the human personality and the possibility of participation in decisions that could mitigate bad conditions; by eliminating the necessity of moral choice, it in effect eliminates morality itself; to this extent self-defeating, it remains a recurrent, perhaps a necessary, dream.

Armed with this model (here starkly oversimplified), Davis is able to shed some fresh light on the familiar landmarks of English utopian thought in the early modern period—More, Burton, Bacon, Winstanley, Harrington—and to bring some lesser figures out of a more or less undeserved obscurity. He argues that these latter constitute an essential body of evidence and deserve a close reading even if they did not themselves occupy an important place in history. This is especially true of some later seventeenth-century writers on whom Davis confers a certain significance in relation to the arcane context of modern full-employment economic theory.

Despite a somewhat repetitious analysis and one that some will find overly detailed, this book remains the best as well as the most intensive treatment of utopian thought in a period when Englishmen were especially given to it.

ARTHUR B. FERGUSON
Duke University

CORINNE COMSTOCK WESTON and JANELLE RENFROW GREENBERG. *Subjects and Sovereigns: The Grand Controversy over Legal Sovereignty in Stuart England*. New York: Cambridge University Press. 1981. Pp. viii, 430. \$39.50.

This illuminating study of English constitutional thought in the mid- and later seventeenth century is potentially controversial. At a time when many Stuart historians are continuing to spurn constitutional history in favor of administrative and micro-political studies and are rejecting Whig or Liberal paradigms of English political history, Corinne Comstock Weston and Janelle Renfrow Greenberg carefully analyze the theories of lawmaking that were publicly espoused by influential Englishmen between 1642 and 1689 and insist on neatly categorizing these theories as either royalist or parliamentary. Adopting a new pair of labels for these positions, they show in great detail how an older "order theory of kingship" that had dominated political thinking before the Civil War was ultimately replaced by the time of the Glorious Revolution by a newer and more "radical" "community centered view of government."

For Weston and Greenberg, as for many older scholars, the central political question raised during these years was not which faction was to rule but "Who makes law?" (p. 3). Was law made, as the "order" theorists maintained, by a king who was the source of all human law and who, while legislating in Parliament, could still legally suspend enacted law with his dispensing power? Or was law jointly created, as the newer theory held, by the Commons, Lords, and king, who together wielded sovereign power in Parliament to make laws that the king could not later set aside? To show how this controversy was carried on for almost fifty years, the authors stage a drama whose principal *personae* are not patrons and clients scrambling for place and position but royalist spokesmen, like Dr. Robert Brady, contending over abstruse points of law and early English history with their parliamentary counterparts, like William Petyt. Instead of seeing this ideological conflict as a sterile war of words that merely lent superficial dignity to crass political power struggles, Weston and Greenberg insist that the growth of a "community centered ideology"—first fully articulated in the early 1640s—effected a radical and lasting change in English political thought and constitutional structure.

Like any clear and uncompromising study, this one is open to several objections, some of them more telling than others. Here and there the authors' textual analyses could be queried, along with their insistence on treating the dispensing power as the central constitutional question of this era, but such criticisms are unlikely to undermine their main

thesis. Their innovative but still traditional argument, however, could be criticized for focusing too much on political "rhetoric" and too little on political action, for overestimating the distance separating royalists and parliamentarians, for underestimating the number of distinct positions taken in the controversy over sovereignty, and for exaggerating the intellectual and constitutional changes wrought by the controversies of this era. In short, Weston and Greenberg could be criticized for adopting a neo-Whig position. They, however, could effectively respond to much of this criticism in two ways: first, by querying the disposition of their critics to trivialize the political struggles of the Stuart era and to dismiss law and political ideas as ideological superstructures erected on an allegedly solid and irreducible base of political interest; and, second, by insisting, along with Quentin Skinner and others, that in showing how certain political positions were conceptualized and legitimated, they have helped to explain political behavior.

Harder to defend is Weston and Greenberg's own deposition to take constitutional arguments at face value and to treat them as being almost self-explanatory. In their laudable attempt to make sense of seventeenth-century political discourse, they say little about the more concrete objectives of participants in the great controversy over sovereignty. They do not really analyze the model of community implicit in their "community centered" ideology. Nor do they explain why this allegedly "radical" theory stopped short of the radicalism espoused by more extreme groups, whose views they largely ignore, or how this same theory was sometimes reconciled with a belief in natural or fundamental law. Nevertheless, while their book does too little to demystify the literature of legitimation that the battle over sovereignty engendered, its lucid and learned analysis of constitutional ideology will have great value for all but the most stalwartly reductionist analyses of seventeenth-century English politics and law.

STEPHEN D. WHITE
Wesleyan University

J. R. JONES. *Britain and the World, 1649–1815*. Atlantic Highlands, N.J.: Humanities Press or Harvester Press, Brighton, England. 1980. Pp. 349. \$31.50.

This is an ambitious book with much unfulfilled promise. J. R. Jones, whose earlier works have encouraged a welcome reordering of thought about English politics from the Protectorate to the death of Anne, now adjusts his gaze to include Hanoverian Britain and the final triumph over Napoleonic France. The purpose is a history neither of domestic politics nor of foreign policy, but an exploration of the interaction between the two. In pursuing this

theme, the first of two, Jones examines the instruments of policy: sea power, threats of invasion and descents upon the enemy's coast, the protection of maritime trade, military power, and diplomacy. Context is provided by the series of wars that began in the mid-seventeenth century against the Dutch and culminated at Waterloo some one hundred sixty-five years later. (The treatment of the three Dutch wars is a commendable example of fresh historical synthesis.)

The magnitude of the first theme not daunting Jones, a second is adopted: Britain's expansion, economically and politically, from secondary status under the later Stuarts to "the first authentic world power in human history" (p. 10).

How did this come to pass? Not through the efforts of a united and cohesive nation (which appeared very rarely, as in 1690 and 1803, in times of gravest crisis). Nor through those of Parliament, whose influence on foreign affairs mounted steadily throughout the period but whose capacity to formulate, to understand, or even to become interested in foreign policy remained low. Nor through monarchs who identified personal desires with national interest. The last two Stuarts made their country a satellite of France. William III thought in Dutch, or, at least, Continental terms. The first two Georges were preoccupied with Hanover. (Only Anne and George III were "mere British.") Nor through ministers and diplomatists, the administrators of foreign policy, who were for the most part inexperienced, ignorant, and hopelessly provincial aristocrats. And not through a government galvanized by the demands of a public that was increasingly vocal, to be sure, but primitive in its understanding about international relations.

All these stock actors in the usual scenario conceal important influences from another quarter: sectional and pressure interests representing mercantile, commercial, and—by the end of the period—industrial groups. A preeminent example is furnished by the East India Company, which pursued its ends with equal success under Stuarts and Hanoverians. In the confusion and conflicting purposes among the ostensible leaders of the nation, such pressure groups were able to exert great—indeed, determining—force. They *knew* about foreign affairs. It was quite literally their business to do so, and for their own benefit they brought economic and political resources to bear on British foreign policy.

The nation was fortunate, Jones argues. Survival in the French Revolutionary and Napoleonic wars was not due to the quality of national leadership; Pitt, Grenville, Fox, and Liverpool are put down as mediocrities. Salvation came rather from an alliance of the government with commercial and new industrial bourgeoisie. Both gained, but Britain the more: its economy did not collapse, and, by extension, it

and Europe were rescued from the threat of a French tyranny.

Lord Acton would have been pleased with the short shrift given here to traditional periodization. The result, however, is only an interesting hypothesis. There are no footnotes. The bibliography is so "select" that the book fails even as a synthesis of recent scholarship. There is an overall and unmistakable impression of haste. The leisure of the nineteenth-century scholar has gone forever, alas; but the themes that Jones wishes to develop are so large that they deserve more time and reflection than they have apparently received.

CHARLES R. RITCHESON
University of Southern California

MARIE PETERS. *Pitt and Popularity: The Patriot Minister and London Opinion during the Seven Years' War*. New York: Clarendon Press of Oxford University Press. 1980. Pp. xiv, 309. \$55.00.

Historians have agreed with contemporaries that the elder Pitt was unique among the eighteenth-century politicians. Lacking either a strong parliamentary base or the king's favor, he nevertheless became the effective head of government from 1757 to 1761 because of his popularity. John Brewer devoted a chapter of his recent *Party Ideology and Popular Politics at the Accession of George III* to examining Pitt's entire career in the light of his unusual base of strength. Now Marie Peters gives us a useful monographic study of Pitt and popularity during his first ministry.

Peters makes it clear that Pitt's popularity was of a special sort—very different, say, from the nationwide backing enjoyed by another great war minister, Winston Churchill. For one thing, the public opinion that really counted in 1757 was not national but that of London. The City was easier to arouse and was accustomed to political agitation; provincial opinion was much more difficult to mobilize. More important, the means available for influencing public opinion were still rudimentary. Churchill could reach millions through radio and mass-circulation newspapers, but in the 1750s the most important medium of political discussion was still the occasional pamphlet. Also, Peters argues, the public's perception of politics was "limited and unrealistic" (p. 272), focused on a small number of familiar topics in a crude and stereotyped fashion.

Given these limitations in both the transmitting and receiving apparatus, as it were, Pitt could not have achieved popularity by fostering a specific public understanding and approval of his unconventional war plans and policies. Public opinion had its shape already fixed, and popularity could be won only by projecting an image that fit a traditional set of prejudices and attitudes *already existing* in the

public mind. To be popular, one had to assume the robe and role of a "patriot" in that word's special, eighteenth-century sense of a selfless "country" or opposition stalwart. Pitt's years in opposition and his outspoken scorn for Hanover suited the requirements perfectly. But to be a patriot *minister*, and moreover one whose war strategy involved campaigns in Germany, was almost a contradiction in terms.

Peters examines the press and politics of the City during the four years when Pitt managed this balancing act. There is little here about Pitt's own involvement in the propaganda campaign; he seems mainly to have left such things to his political allies. We also could wish to know more about the ways in which concern for popularity actively influenced or constrained Pitt's conduct of the war. But Peters is both more interested and more authoritative when dealing with London politics and opinion than with the war itself. (In her index, Alderman William Beckford has 101 entries, James Wolfe none at all.)

The body of the book is for specialists, then, although the excellent introduction and conclusion, which are more general, should be read by anyone with an interest in eighteenth-century politics and public opinion. The footnotes are where they belong, at the bottom of the page; but then, given the price, they ought to be.

THOMAS W. PERRY
Boston College

GEORGE SHELTON. *Dean Tucker and Eighteenth-Century Economic and Political Thought*. New York: St. Martin's Press. 1981. Pp. 289. \$25.00.

Josiah Tucker was born in 1713 in the small Welsh village that was later to produce the poet Dylan Thomas. His early circumstances were so obscure that he may have had to walk the hundred and sixty miles to Oxford when he went up to university. It is certain that he was so poor he could not go before he was twenty. Four years later, a priest, he became curate of St. Stephen's in Bristol. There he married a prosperous widow who was seventeen years his senior; there he began to attack Methodism. Over the coming years he wrote against Charles Edward Stuart, the poor, gin drinking, and the torture of chickens. At Cambridge we used to have a code word for people of such exemplary dullness, and that word was *worthy*. Write a letter in favor of the admission of a "worthy young man," and you had destroyed his chances forever.

The young Tucker was certainly worthy enough. He wanted to multiply the property qualification for voting by ten, from forty shillings to twenty pounds, in order to take care of the changed value of money since 1429. He believed that immigration should be encouraged, so as to lower wage rates. Taxes on

bachelors should be tripled in order to encourage prosperity by means of a baby boom: "don't vote, populate." Tucker even suggested building houses on stilts in marshy areas, so that the living quarters would be safely above any unhealthy mists. All this was a curious mixture of the practical and the merely odd. Tucker was promoted, neither for his opinions on economic questions, nor for his services to religion, but for his work in helping the political life of Robert Nugent. Newcastle made Nugent a treasury lord and Tucker, in time, dean of Gloucester. By this time he had advanced to the position of rector of St. Stephen's, and for many years after 1758 he commuted between the two cities.

It would not be fair to say that Tucker's reputation was higher in France than in Britain. It is true that some of his pamphlets were translated by Turgot and that from 1755 his work was known in print to the physiocrats, but by then he was also known to Hume, whose work he influenced, and to the Prince of Wales. At one time he thought of writing his "big book" as a text for the instruction of the later George III. But the book never appeared under Tucker's name. Adam Smith used some of his ideas; Shelburne and the younger Pitt used them too; they all used the ideas without giving the dean enough credit to satisfy the author of this study, George Shelton.

Tucker opposed war, and particularly wars of imperialist conquest, on the simple ground that they did not pay. Armies and navies would never survive a cost benefit analysis conducted by the dean. He felt this way about the Seven Years' War before it began; he felt this way about fighting to retain the American colonies from as early as 1766. It was not that he particularly liked Americans—he thought that Northerners were fanatical hypocrites and Southerners tyrannous slaveowners—but he believed they were not worth the expense of fighting for. Tucker's views on Ireland and on women are also interesting. From as early as 1749, he wanted to incorporate Ireland into Great Britain, and he could see no particular reason why women should not vote. This led him to oppose the radicals, who were only interested in manhood suffrage, and to denounce the memory of Locke. Indeed, one of Tucker's reasons for wanting to let the Americans go was his fear that democracy in America might lead to democracy in Britain. He came to hate slavery, but he never changed his mind about the mob. The French Revolution, which he watched before his death in 1799, would have reinforced his opinions on that subject.

Tucker's work reflected his western location. Bristol suffered from the wars of the eighteenth century and was badly weakened by the loss of the American colonies. In recent years his reputation has been upheld at Columbia and denigrated at Princeton.

Shelton gets rather too involved in that now ancient squabble between Jacob Viner and R. L. Schuyler, and it must be said that his bibliography is a bit out of date. For the rest, this is a useful introduction to the work of a neglected figure.

STEPHEN BAXTER

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MICHAEL TURNER. *English Parliamentary Enclosure: Its Historical Geography and Economic History*. (Studies in Historical Geography.) Folkestone, England: Dawson or Archon Books, Hamden, Conn. 1980. Pp. 247. \$22.50.

The end of geography is a map, the end of history a story, the end of economics a model that fits both. The three are not always compatible, and Michael Turner's attempt to meld them is therefore ambitious. Although not on every count successful, the book is a useful one.

Its chief usefulness is that it places the county totals of numbers and amounts of parliamentary enclosure in England on as firm a base as one historian's energy can be expected to construct. The appendixes to the book amount to a compilation from Turner's own editing of the posthumous book by W. E. Tate, *A Domesday of English Enclosure Acts and Awards* (1978); the text of the book amounts to a commentary on and use of the appendixes. The conclusions are not strikingly novel in outline: the enclosure movement was concentrated in a great triangle centered on the East Midlands and came in two twenty-year spurts before and after the 1780s. But we know these conclusions and are on altogether firmer ground than that on which the first professional historians of enclosure stood some seventy years ago.

The book is also useful for its fine pair of chapters explaining the rate of parliamentary enclosure. These reach very high standards of econometric sophistication and historical candor. Turner goes so far as to actually reproduce the data for his regressions, a procedure quite natural to the historian but unheard of in economic circles, where it is thought wiser to bury the weapon after the murder is committed. The conclusion is, again, not shocking: interest rates were important to investors who contemplated spending thousands of pounds on an enclosure. What is more important is the careful skepticism with which the various arguments are reviewed.

English Parliamentary Enclosure is not an especially skillful piece of historical geography. In the last chapter, for instance, Turner claims to substantiate an oft-repeated assertion that a shortage of grazing land before 1750 prompted the enclosures after 1750, but the evidence for a connection is not there.

Those wanting a geographical treatment of the enclosure movement will turn instead to recent books by Yelling and others (the field has become crowded of late). Yet Turner has spoken the last word on the county statistics of enclosure by act of Parliament, or at least the last word for another seventy years.

DONALD N. MCCLOSKEY
University of Iowa

JOHN ROACH. *Social Reform in England, 1780–1880*. New York: St. Martin's Press. 1978. Pp. 256. \$25.00.

John Roach argues that "social reform began in Britain in the age of the French Revolution" (p. 18). It is a bold claim. Was not that age one of repression and reaction? Where in those years were the monumental acts of social reform of the 1830s and 1840s? Where were the new poor laws, education committees, and the factory, mine, prison, lunacy, and health acts?

The absence of such acts does not bother Roach, since he sees social reform in the acts of individuals and localities, in improved cottages, Sunday schools, new prisons, and ampler (or stricter) poor relief. Such activities, however, were long part of English life. They did not "begin" around 1789. Neither would most of those paternalists and philanthropists who helped the poor admit to a desire to reform society. For them poverty was inevitable, sin ineradicable, and subordination the main object of schooling.

The early Victorian social reformers had moved far beyond these static ideals. Roach is chary about admitting this fact, claiming that the great reforms of the 1830s and 1840s were revolutionary only in "pace" and "scale" (p. 89). It is again a bold claim. From 1833 to 1856 the early Victorians created some eighteen central authorities with hundreds of inspectors and with powers—judicial, legislative, executive—that were vast, unprecedented, and revolutionary.

Roach's 228-page survey is largely a series of summaries of government reports and legislation on social questions interspersed with biographies of reformers and *précises* of major thinkers. There is no integrated analysis. In his preface, Roach says that "the purpose is not primarily to record facts and events," but to search for the "ideas," "motives," and "causes" behind the reforms (p. 22). But except for some conventional references to evangelicalism, political economy, and Benthamism, the book is largely a record of facts and events, mostly unrelated to each other.

Some of the events are also not even too relevant. Why so much on civil service and university reform and on Chartism, the Great Exhibition, and trade unions when there is not one word on lunacy? And

why 4 of the 228 pages on the obscure Jelinger Symonds? In a moment of candor, Roach confesses that he included the Symonds sketch because "generalizations . . . are difficult to make" (p. 99).

They are indeed, particularly generalizations that integrate historical facts into a unified analysis. Such generalizations are absent in Roach's study. Instead, generalizations are now and then spun off, some of dubious validity. That Thomas Malthus was a social reformer and Robert Owen a political economist should surprise many. Historians of the United States and Holland will not believe that Britain around 1800 was "the only capitalist power of the modern kind" (p. 60), nor will historians aware of illiteracy in England believe that political economy and utilitarianism "affected everyone" (p. 46). No more believable is Roach's claim that philanthropy "implies the direct contact between donor and recipient," a claim at odds with the fact that the donors and recipients in the greatest of philanthropies—those to emancipate slaves and to convert the heathen—were thousands of miles apart. Because of its lack of clear distinctions and unifying analysis and the dubiousness of some of its claims, Roach's survey is of no great value.

F. DAVID ROBERTS
Dartmouth College

KAREL WILLIAMS. *From Pauperism to Poverty*. Boston: Routledge and Kegan Paul. 1981. Pp. 383. \$60.00.

This collection of essays on the poor laws and the leading commentators on poverty in nineteenth-century England is held together by a powerful thread of disdain for the work of others. The very title, Karel Williams informs us, "is intended as an ironic comment on the obsessions of other historians" (p. 5). Throughout the book, Williams maintains a vigorous assault upon the presuppositions, methods, and findings of every significant historian of the subject since the Webbs. His aim is not simply to rectify errors in a particular field, but rather to challenge the dominant modes of Anglo-American historiography.

Williams divides his treatment of the poor law at the customary date of 1834, stressing both the discontinuity of relief practices between the Old and New Poor Laws and the different methods employed by scholars specializing in each period. Historians of the Old Poor Law, it seems, are wedded to an empiricist approach involving testing procedures of inappropriate social science models. Thus Mark Blaug's use of marginalist economic theory to show that the allowance system was in marked decline by the 1820s is condemned because marginalism can apply only to "elegant trivialities" and "cannot define determinative results and connections for large-scale problems involving complicated change across

a whole economy" (p. 31). Furthermore, Williams criticizes Blaug's use of the relief statistics of 1824 and 1832. By taking a fresh look at the categories used in those returns, he is able to make a compelling case that allowances to the able-bodied and their families were the most important component of poor relief right up to the passage of the New Poor Law.

Turning to the New Poor Law, Williams heightens the 1834 discontinuity by asserting that the reformed system succeeded by the 1840s in all but eliminating outdoor relief to able-bodied men and their families. This interpretation is of course in stark contrast to that of most modern historians, who, Williams claims, have confused the category "able-bodied persons" with "able-bodied men." This confusion, together with their use of a decidedly old-fashioned narrative mode and reliance on anecdotal material, has led them to misinterpret egregiously the nature of the Victorian poor law. Historians of the post-1870 system are criticized for fragmenting the material into sub-topics like pauper education, medical relief, and vagrancy, thus misconceiving or overlooking the new indoor strategies and workhouse designs of the late nineteenth century.

In similar fashion, Williams criticizes modern analysts of Victorian social investigators for their attempts to assess their subjects' work in terms of its factual correctness or its approach to modernity. It is in this part of the book that Williams makes the most effective use of an alternate approach in the form of a historiography adopted from modern linguistic theory, particularly the work of Roland Barthes. What is involved is an insistence upon multiple readings of a text with full sensitivity to ambiguities of words and classifications to produce a "polysemic" view. Thus freed from a concern for causal linkages and empirical models, Williams is able to offer some fresh and provocative readings of the classic texts on poverty. Mayhew is seen as articulating a decidedly presocialist "respectable artisan" point of view, while Engels's text (the most polysemic of the lot) is at once a morally charged attack on the villainous bourgeoisie, a work of classical economics, and a humanist tract on alienation. Williams attacks those who allege the modernity of the Booth and Rowntree texts by demolishing the former's elaborate taxonomy of poverty and the latter's "poverty line" calculations.

There is much more that is stimulating and controversial in Williams's offbeat, quarrelsome, and extremely important book. If it is taken as seriously as it deserves to be, it could inspire a vigorous and valuable debate not only about the history of poverty but also about the very nature of history.

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DAVID VINCENT. *Bread, Knowledge, and Freedom: A Study of Nineteenth-Century Working Class Autobiography*. London: Europa. 1981. Pp. 221. £18.00.

Readers who are familiar with nineteenth-century British labor history sources will recognize the title of David Vincent's book as belonging originally to William Lovett's 1876 autobiography. Others must read 109 pages before encountering the author's confession. Indeed, the reader will search in vain for exciting revelations or confident interpretation. Instead, Vincent diligently and competently elaborates and confirms many of the findings of the new social history. In so doing he achieves his purpose of discovering how working men and women perceived their existence and opens slightly wider the window into their world.

Bread, Knowledge, and Freedom is a revised and expanded Ph.D. thesis based on 142 artisan autobiographies. To be included in the study the autobiographies had to be genuine, not fictional; the autobiographer had to have been a member of the working class; and the autobiography had to cover some segment of the period 1790–1850. As such, the subtitle of Vincent's book promises more than he delivers, although occasional references are made to the post-1850 period and the bibliography includes a section of working-class autobiographies for the last half of the century.

In part 1, Vincent analyzes the problems and potentials of using working-class autobiography. In part 2 he describes the working-class family as presented in the autobiographies studied. Part 3 consists of an analysis of the concept of "useful knowledge," and part 4 is a conclusion. This well-written book is based on solid research. The author makes judicious use of quotations throughout to reveal the flavor of life as seen by the autobiographers. The footnotes and the bibliography will lead the reader to a considerable number of the recent writings on social history, as well as to the working-class autobiographies themselves.

Vincent argues that "the pursuit of knowledge derived its impetus from the circumstances under which it was gained, and in turn was seen as the essential pre-condition for the pursuit of freedom" (p. 109). For the elite group studied, he substantiates that claim. It is necessary, however, to remember that his sample is not a true cross-section of the working class for the period studied: Vincent's subjects were literate, came predominantly from nonindustrial skilled trades, concentrated in urban centers, and with six exceptions were men. If they measured progress in terms of intellectual and political freedom, they were aberrations from the working-class norm. The mass of the working class measured progress—when they bothered—in terms of bread. In their vision, knowledge and freedom were caught between the material conditions of

present-day life and those hoped for in the future. Vincent's study serves as a reminder of the chasm that existed between some members of the upper stratum of the working class and their unskilled brethren. That may even be a partial explanation of the gulf that existed between the artisan autobiographers' "faith in progress and their continuing private troubles" (p. 200).

Yet this study is worth reading, for it contains considerable information. Its tenuous nature suggests that it is a stepping stone to a study of broader and firmer interpretation. Perhaps the promised full annotated bibliography of nineteenth-century working class autobiography being prepared by Vincent and John Burnett will point that way.

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D. G. PAZ. *The Politics of Working-Class Education in Britain, 1830–50*. Manchester: Manchester University Press. 1980. Pp. xi, 203. \$30.00.

Like most aspects of modern British society, the practice of state-supported education is firmly rooted in the Victorian era. Several recent monographs and scholarly articles attest especially to the importance of education for women and working-class children in nineteenth-century Britain. To the latter group *The Politics of Working-Class Education in Britain, 1830–50* attends, but barely. D. G. Paz is little interested in the social origins, forms, or effects of working-class education. His focus is primarily on the cumulative political-administrative process of educational reform as it was carried out, largely at the cabinet level, from the days of the Great Reform Bill to the death of Robert Peel, when the Victorian state increased its financial commitment to education from a mere £20,000 to £125,000 per year.

It is an unheroic story of educational clauses attached to factory bills and poor laws, endless debates between pressure groups and administrative factions, and petty squabbles among various religious interests and school societies. Flitting back and forth across the stage are such colorless characters as Thomas Spring Rice, Henry Petty-Fitzmaurice Lansdowne, and J. S. W. Wharnccliffe—moderate, practical politicians all. In this narrative, Lord John Russell and Sir Robert Peel similarly lack flesh and blood. Whigs and Tories are virtually indistinguishable. All seem to be men of few ideas and fewer principles.

This is the stuff of the bureaucratic "nineteenth-century revolution in government" controversy of a few years ago. Like Oliver MacDonagh, Paz minimizes the importance of Jeremy Bentham and the Utilitarians as sources of political inspiration or programs. He mentions Irish, Scottish, and Continental models of educational policy but in the end offers no explanation of either the ideological or

emotional content of English reform. State-supported education was certainly a most emotional, controversial issue. Why, then, did those elite guardians of the body politic contend so earnestly with peers and prelates when hammering out their compromises? Paz does not say. Instead, he concludes with the tautology that "policy-making was essentially a political activity" (p. 149). Meticulous in research, he carefully delineates the course of that policy-making only to smother its fire under a bureaucratic blanket.

Paz is of the opinion that his research suggests the need for some reappraisal of the social control hypothesis. Yet the notion of social control is by no means logically antithetical to bureaucratic growth. Paz himself quotes, then ignores, a Poor Law commissioner's observation that those working-class children who were the best educated were also the most orderly, honest, industrious, thrifty, and prosperous. According to Paz, James Phillips Kay, the first permanent secretary of the Education Department, believed "the first task of education" to be the eradication of "the influences of the home and neighbourhoods characterised by drunkenness, filth, crime, instability, and irresponsibility" (pp. 56–57). But Paz passes quickly over these impediments to what Himmelfarb calls "other men under the impulse of other ideas." Unfortunately these "other men" are both uninspired and uninspiring, and the "other ideas" are unclear if not nonexistent.

Despite all his anti-Whig assumptions, Paz establishes two fundamental points of "progress." Between 1830 and 1850 Anglican leaders reluctantly assented to the proposition that other denominations had the right to receive public funds for the education of their children. More importantly, Victorian government moved rapidly from its timid resolve to a firm commitment to state support for education. These were no small accomplishments.

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CHARLES MORE. *Skill and the English Working Class, 1870–1914*. New York: St. Martin's Press. 1980. Pp. 252. \$22.50.

Charles More's study calls into question the deskilling thesis—the argument that the expanding logic of industrializing capitalism brought about the diminution of skilled labor by the start of the twentieth century—and presents itself as a refutation of the two major contemporary accounts of the relation between capitalist development and skill. Well before the end of the nineteenth century, according to H. A. Turner, skill differentials and apprenticeship as a method of acquiring skill persisted in most industries not as genuine requirements but as social constructs imposed by unions as part of an effort to

restrict the labor market and thereby artificially raise or protect wages and job security. By accounting for the survival of skilled labor in this way, the social constructionist argument presumes the overall decline of the objective basis of skill. This presumption is shared by Harry Braverman, who claims that the deskilling of the labor force was a strategy carried out by management to extend its control over the productive process. To the extent that skill differentials prevailed, they did so in the form of artificial job ladders designed to impede concerted worker opposition. More argues that both accounts are mistaken: Turner's in important places, Braverman's in its totality.

Skill, for More, consists of some combination of manual skill and knowledge and is operationalized in the training time needed to acquire it. More's inquiry into the necessity of skill concentrates on changes in skill requirements in the British engineering industry between 1870 and 1914. Drawing from workers' assessments of their training that appear in the "Family, Life, and Work" survey and in worker autobiographies and from official reports and social science investigations concerned with training, he begins his inquiry by identifying the dominant methods available for the acquisition of skill.

Three primary methods of skill acquisition prevailed during this period. The method of regular service or apprenticeship differed from migration, learning by moving from shop to shop or from machine to machine, and following-up (where a gang member learned the job of the man above while carrying out his own full-time duties), in that a specific time was set aside for learning, the learner sacrificed potentially higher wages for the duration of the apprenticeship in return for a promise of future reward, and the importance of high quality was emphasized explicitly throughout the process. Apprenticeship, while not the only route to skill acquisition, clearly was the most important throughout much of the period in question.

More's findings suggest that the survival of apprenticeship reflected neither union strength nor managerial control strategies but the objective requirements for genuine skill. While aspects of apprenticeship were often arbitrarily defined, they bore the influence of custom much more than of union will. Indeed, More finds no evidence to support the view that unions possessed the wherewithal to force apprenticeship on resisting or indifferent employers. Apprenticeship was maintained for more pragmatic reasons. Employers saw it as a cheap and effective method of training, one that tended to strengthen worker commitment to the firm; workers regarded it, justifiably, as a valuable and sure way of obtaining considerable skill and enhancing their future prospects. Given that the

market for British engineering products favored diversified over standardized output, technology was employed that demanded the versatility of skilled labor. In engineering, More discovers, while some groups experienced a slow, steady decline in manual skill during this period, most groups found that they were expected to have higher levels of knowledge and intelligence.

Generally, there is little support for the thesis of unilateral skill decline. The ineffectiveness of World War I dilution efforts—the attempt to have unskilled and semiskilled workers perform the work of the skilled—indicated that workers with genuine and considerable skill were indispensable at least up to 1914. As More sees it, the development of industrial capitalism brought about not so much a process of deskilling as one of industrial change in which the number of jobs in particular industries and occupations declined but the character and skill level of most jobs remained more or less stable. More specifically, in those trades where skill level declined as a result of mechanization, frequently the skill was not very substantial in the first place. Where it was, mechanization usually produced subdivisions in the form of specialization (delimiting the job to increase skill requirements), not routinization. As a consequence of these industrial changes, old trades were replaced by industries in which migration and following-up were the primary methods of skill acquisition. Not a decline in skill requirements but a shift in the mode of acquiring skill was the key change in this period.

Accordingly, More argues that the decline of the labor aristocracy is attributable not to deskilling but to a blurring of the lines of demarcation between skilled and unskilled that occurred with the displacement of apprenticeship by migration and following-up. The growth of non-apprenticed skill work and, with it, the rise of all-trades unions diluted the labor aristocracy. Thus, the diminishing status of the labor aristocracy represented—and this seems to be a fair summary of More's general thesis—more a strengthening of the weak than a weakening of the strong.

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DAVID CANNADINE, *Lords and Landlords: The Aristocracy and the Towns, 1774–1967*. Leicester: Leicester University Press; distributed by Humanities Press, Atlantic Highlands, N.J. 1980. Pp. 494. \$49.50.

David Cannadine's major volume is the culmination of many years of investigation of British aristocratic estate papers, and it is concerned with the impact of great and noble landlords upon certain aspects of urban society. Sandwiched between five interpretive

chapters dealing with the problems of aristocratically influenced Victorian towns in general are two detailed case studies: one of a middle-class suburban retreat, Edgbaston in Birmingham; and the other of the superior seaside resort, Eastbourne in Sussex.

The case studies are well chosen in that they produce areas of contrast as well as points of similarity, and Victorian "planning" is seen in unusually close and well-documented focus. Edgbaston was steadily developed by the lords Calthorpe over several generations, whereas Eastbourne was created under the tutelage of William Cavendish, seventh duke of Devonshire. But the fifth Lord Calthorpe, a free spender as well as an astute investor, saw to it that Edgbaston was most rapidly developed when Eastbourne burgeoned, principally in the years following 1870. The duke of Devonshire, temperamentally a very different individual, was also engaged in recouping his family's fortunes through some initially successful investments in the totally new town of Barrow-in-Furness, and he was accustomed to permitting unusually capable local helpers to take detailed decisions for him. Both Devonshire and the Calthorpes had admirable agents, named Wallis and Edwards respectively, and they in turn were greatly assisted by family firms of lawyers attached to the respective aristocratic houses. Cannadine takes full advantage of the family business papers concerned to throw some light on complex decision-making processes.

Inevitably there are many lacunae; nor does this surprise the present reviewer, who has struggled with similar material. The duke of Devonshire distanced himself from many of the worries of town planning and was greatly concerned to make local residents pay for the improvements within the towns on his territory. The earlier Calthorpes, who had enjoyed much esteem in Birmingham itself, did not have to face the effects of radicalism in the mid-Victorian city, nor did they live to see their leafy estate invaded by tramways and occupiers of poorer-class housing. By the end of the century, town councils were—in places besides Eastbourne and Edgbaston—challenging ducal or aristocratic power, but they were also awarding mayoralities to local noblemen by way of compensation.

Estate-office control of new towns or suburbs could be powerfully motivated by preoccupation with the maintenance of "tone," through countless individual decisions as much as by the use of planning regulations. All went well as long as the markets for superior housing and park-like environments were not threatened by exterior economic and social influences; and, in this respect, one would have wished to know more about the speculative builders who were encouraged to carry out much of the work. Cannadine's more general chapters on "theory and argument" in British urban history are

well and cogently written, but they are somewhat vitiated by the past and current weaknesses of the literature within this field. Regrettably, few published writers on British towns and cities have approached their topics with Cannadine's thoroughness and imagination, and he is obliged to cite works that are pretentious or inconclusive. He is also inclined to repeat himself at considerable length. Nevertheless, his points fully deserve emphasis, and this is an outstanding book.

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RICHARD A. COSGROVE. *The Rule of Law: Albert Venn Dicey, Victorian Jurist*. (Studies in Legal History.) Chapel Hill: University of North Carolina Press. 1980. Pp. xv, 319. \$19.50.

Richard A. Cosgrove's book on Albert Venn Dicey (1835–1922) is the first comprehensive study of that significant figure who long dominated university study of the English law and constitution. Historians once accepted as canonical Dicey's version of the relationship among political forces, public opinion, and legislation in nineteenth-century Britain. Although his dichotomy between "individualist" and "collectivist" legislation now tends to be rejected by professional historians, these terms persist in political discourse and continue to play their part in neoconservatism.

This book is based upon exceptionally thorough research, much of it in private papers. Cosgrove carefully describes, analyzes, and criticizes Dicey's books and political attitudes. No doubt the distinctive and excessively constricting emphasis of the book derives from its publication in a series called "Studies in Legal History." Cosgrove covers in detail Dicey's technical works, such as *A Digest of the Law of England with Reference to the Conflict of Laws*; his *Introduction to the Study of the Law of the Constitution*, which is in part legal and in part political; and his *Lectures on the Relation between Law and Public Opinion in England during the Nineteenth Century*, his most influential political book and certainly the best known to historians. Cosgrove emphasizes as his own contribution that long section dealing with Dicey's work as a Liberal Unionist publicist fanatically opposed to any form of home rule for Ireland. Certainly there remains much to ponder for anyone concerned to understand how that issue permanently split the Liberal party and very nearly led to civil war in 1914. However, Cosgrove does little to analyze—much less explain—the ideological quality of Dicey's obsession.

Dicey's activity as a professional jurist was lifelong. He rigidly adhered to the analytical, antihistorical theory of law championed by John Austin. No aspect of utilitarianism is more obscure to nonspe-

cialists than the effects of the campaign to simplify and codify the common law. Cosgrove is right to emphasize Austin's method as determining the nature, direction, and limitations of Dicey as legal theorist. But in the absence of adequate comparative and philosophical contexts, Cosgrove's discussion often misses the most significant points about this aspect of Dicey's career. Although he considers in detail Dicey's notorious dismissal of French *droit administratif* as incompatible with the rule of law, Cosgrove fails to compare Dicey as legal theorist to such Americans friends as Oliver Wendell Holmes, Jr. Nor does Cosgrove seem to be aware of what in Germany were the consequences of a positivist and analytical theory of law executed according to the prescriptions of Austin and Dicey.

By adapting Austin's jurisprudence to ends less reformist and more conservative than those envisioned by the first English utilitarians, Dicey was able to bring together the academic study of law, politics, and opinion into a synthesis so abstract and formal as to make these subjects remote from the dilemmas of choice faced by statesmen, parties, and citizens. Thus insulated from reality, Dicey turned his mid-Victorian liberalism into a judicial and political creed that declared virtually all social legislation to run contrary to the rule of law as he defined it. (The line taken by the United States Supreme Court prior to the New Deal resembled Dicey on this point.) Cosgrove provides an interesting case for arguing that utilitarianism could be, and indeed was, taken in an antireformist direction—that of Dicey rather than that of John Stuart Mill. He treats Dicey as a thinker incapable of going beyond his original mid-Victorian liberalism. Yet on the basis of the evidence offered in this book, another conclusion seems more plausible: Dicey gave up Mill for Austin, and in the process transformed utilitarianism. In his *Law and Public Opinion* (based on lectures given at the Harvard Law School in 1898), Dicey provided a specious interpretation of utilitarianism as an originally individualist, libertarian, and anti-interventionist creed that had been replaced by an indiscriminate collectivism mistakenly committed to doing good on the basis of a merely sentimental concern with suffering. As a theorist of the constitution, Dicey argued that its distinguishing characteristics were the legislative sovereignty of Parliament, the rule of law, and the observation of conventions not themselves legally enforceable. Negatively, Dicey asserted that in England there was not, nor could there be, any system of administrative law outside the ordinary courts. Taken together, these arguments constituted a narrow legalism that condemned the new liberalism.

Cosgrove ends his book by citing Laski's judgment that, as an Oxford professor, Dicey exerted more influence upon the outside world than any other

teacher there since T. H. Green. Green had rejected utilitarianism because he believed its implications were ultimately incompatible with liberty and liberalism. Today many moral and political philosophers contend that utilitarianism, even in its most refined modern forms, cannot provide adequate protections for the rights of individuals and minorities. Had Cosgrove chosen to explore the significance of Dicey's thought for the development of utilitarianism and liberalism in British thought and politics, he would have added a further dimension to his praiseworthy study of his subject's place in the history of English law. His book will still be required reading for legal and constitutional historians. As the first rounded study of Dicey, it will also be informative and useful to general and intellectual historians of Victorian and Edwardian England.

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CATHERINE CLINE. *E. D. Morel, 1873–1924: The Strategies of Protest*. Belfast: Blackstaff Press. 1980. Pp. 180. £9.95.

Catherine Cline has written a detailed but short account of the public life of E. D. Morel. She describes Morel's development from a shipping clerk in Liverpool to a journalist concerned with Europeans' policies in Africa; the formation of his point of view out of Quaker humanism, free trade, and the anthropological theories of Mary Kingsley; his public campaign against King Leopold of Belgium's system of forced-labor concessions in the Congo; his criticisms of "secret diplomacy" before World War I; the conduct of the Union of Democratic Control during and after that war; and Morel's career in the Labour party until his death in 1924.

Cline provides a balanced assessment of Morel. She finds him self-righteous, vain, and secretly ambitious but realizes that an element of self-interest does not automatically make a moral crusader hypocritical. She does not make Morel's career-building into his sole motive. She only leaves room for his humanitarian impulses, however; she does not give the reader a positive sense of what Morel believed in. Since Morel reached his audiences by expressing shared moral concerns, the failure to describe his values makes it impossible for the reader to see exactly how he moved people or to understand the enthusiasm he aroused. Cline does a good job of describing Morel's relationships with his benefactors and close collaborators. Although Morel's vehemence and his ill health after World War I might justify psychological explorations, Cline refuses to guess at psychological sources of his acts because of a shortage of direct evidence.

Cline does not want to exaggerate Morel's importance. She sees his campaigns as personal triumphs but not as lasting contributions to such problems as creating a democratic foreign policy or ending colonialism. She does not feel that he added much to our understanding of World War I. Her account of Morel's impact on Labour's foreign policies expands her earlier discussion in *Recruits to Labour*, but she does not feel that he had a lasting effect. Here her book gives a clear sense of the alternatives to Morel's ideas; Morel ignored the possibilities presented by the League of Nations. Cline sensibly does not claim a direct connection between Morel's hostility to the Treaty of Versailles and Britain's slow response to the Nazis. One wishes that she had told us whether Morel had any interesting responses to pacifism and the shop stewards movement during World War I, the Russian Revolution, and Gandhi and other anticolonial agitators. Perhaps Morel had no such reactions and was only the narrow crusader that Cline describes.

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JO VELLACOTT. *Bertrand Russell and the Pacifists in the First World War*. New York: St. Martin's Press. 1981. Pp. ix, 326. \$25.00.

Like so many of his contemporaries, Bertrand Russell was shocked and disillusioned by the outbreak of war in 1914. His liberal optimism shattered, he groped for new ideas and sought new activities. By war's end, as Russell asserts in his autobiography, he had attained a new philosophy and a new youth. He was no longer the don or the Puritan. This self-assessment serves as a focus for Jo Vellacott's *Bertrand Russell and the Pacifists in the First World War*.

Vellacott, a historian at Acadia University and consultant to the Bertrand Russell Editorial Project at McMaster University, concisely answers two general questions: what were Russell's day-to-day activities during the war, and how did he change in both personality and thought? Until early 1916 his unwavering opposition to the war did not find a satisfactory outlet in writing or in the activities of the Union of Democratic Control. Then, spurred by the passage of the Military Service Act, Russell found in the No-Conscription Fellowship a means of translating his stance against militarism into action. Eleven of the book's sixteen chapters examine his role in the NCF: lecturing, lobbying, writing, and finally in 1917 serving as acting chairman of the organization.

Useful as a case study of the intellectual in politics, Vellacott's book also adds to our knowledge of the antiwar movement by shedding light on the leading personalities, disputes, successes, and failures of the NCF. While noting that Russell was often dispirited by his failure and that of the pacifists to lessen the

prevailing militarism, she does not accept his self-indictment in the *Autobiography* that all that he had done had been totally useless. It was most fully in his work for the NCF, she argues, that Russell became a political and public man, leaving the don behind.

This work also helped Russell to leave the Puritan behind, to deepen that appreciation of emotions he had developed since meeting Lady Ottoline Morrell in 1911. He gained greater toleration of human foibles, more maturity in personal relations, and clearer understanding that passion was essential to human happiness. Taking her key from Russell's statement that the war altered his thinking about man, Vellacott asserts that the NCF was a "forcing-house for learning about human nature" (p. 244). In it he learned that intellectual argument alone could not bring about social betterment, and from this followed a more sophisticated view of politics that Vellacott succinctly summarizes in her concluding chapter.

In giving us the most thorough account available of Russell's public life during the war, Vellacott's sympathy for the man and for pacifism has not led her to exaggerate the importance of his wartime activities. Nor does she fail to see that these years did not begin or end Russell's emotional and intellectual growth. Yet, for him the Great War was a watershed; he emerged from it changed in both head and heart. Vellacott demonstrates that Russell's involvement with the pacifists was essential to that metamorphosis.

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STEPHEN ROSKILL. *Admiral of the Fleet Earl Beatty, the Last Naval Hero: An Intimate Biography*. New York: Atheneum. 1980. Pp. 430. \$19.95.

An old sea dog and a renowned writer, Stephen Roskill—the official British naval historian of the Second World War and an authority on the interwar years—has written an admirable biography of the hoped-for Nelson of the North Sea in the 1914–18 war. Roskill has produced a book that is propelled by triple screws. The first pushes the story of Beatty the man and the admiral, the lover and the commander, the hero but not the glorious victor. The second thrusts along the controversial naval story, setting the man into the developments of the day and with a true sailor's capacity enlightening the reader as to what was happening technologically and nautically. The third screw forces the reader along historiographically with numerous references not only to the controversies, especially after Jutland, but also to the works of other historians, notably that of the highly esteemed American authority on the Royal Navy, the late Arthur Marder.

The biography thus steams across time at a fair 21 knots, like Beatty's battlecruisers, aided by a few footnotes floating at the bottoms of the pages to provide needed explanations and by a following breeze of endnotes to keep the smoke of confusion from the reader's binoculars.

Roskill weighs anchor with a most instructive foreword and acknowledgments, which will open the eyes of many a graduate student to the problems of modern biography: sons wish to protect fathers, but grandsons do not. Thus this is the first biography to discuss Beatty's love life and to note openly that he had more in common with Nelson than just dash and rank. The early life is passed over quickly, although the love of hunting, the family connections, and his wife's wealth are not, for they had a bearing on Beatty's later career. Nor, once he has cleared the shallows and got the ship to sea, does Roskill soften his judgments. He faults Beatty personally for his poor staff work, especially for signals that were at critical moments unclear, and for his lack of the Nelson touch, so that his captains did not know instinctively what he wanted done at critical junctures. Both of these weaknesses led to recriminations and controversies that could have been avoided with a better knowledge of history and more forethought. At the same time, Roskill questions the Royal Navy's technical expertise, pointing out that its real problems in the North Sea included not just poor shells but also inefficient range-finders and firing techniques, weak armor, and poor ammunition discipline.

Finally, Roskill faults the last naval hero for his failure, after the end of the war, to appreciate what the formation of the RAF meant and to get the Royal Naval Air Service (later the Fleet Air Arm) back under naval control before it was too late. By the time Roskill gets to the postwar world he has run beyond the channel buoyed out for him by Marder's previous work, which he acknowledges and criticizes like a navigating chart in steering his own course, and is into the waters for which his own two-volume study is the chart.

This is a readable biography that is both a beginning and a summation on the Grand Fleet in the Great War.

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MARK SWENARTON. *Home Fit for Heroes: The Politics and Architecture of Early State Housing in Britain*. London: Heinemann Educational Books. 1981. Pp. viii, 216. £14.50.

Local authorities have had powers to provide rented housing for working-class people in Britain since the 1860s, but only in the 1900s did public sector housing become a serious political issue. Between

1914 and 1918 there were several pioneering experiments in providing public housing for munitions workers, most notably on the Well Hall and Gretna Green estates. But not until the end of the war did the central government give support to a large-scale local authority building program. Faced in 1919 with an acute housing shortage, Bolshevism in Europe, and the problems of re-integrating four million demobilized soldiers, the Lloyd George coalition committed itself to financial support for 500,000 local authority homes. This commitment was specifically political in purpose: it was designed to divert disgruntled ex-soldiers from revolution and to ensure "the stability of the state." The outcome of this strategy was the Housing Act of 1919, the first of its kind to make available Exchequer subsidies for local authority housing.

The political details of this act and its subsequent shipwreck in the slump of the 1920s are fairly well known. The originality of Mark Swenarton's study lies in his attempt to relate the Housing Act to the context of housing design. Professional views on working-class housing were divided during this period into two distinct architectural camps: the romantic "cottage" school, strongly influenced by the Viennese Wagnerian, Camillo Sitte; and the neo-Georgian functionalist school, headquartered at Liverpool University. Other nonaesthetic factors also reacted upon contemporary approaches to design. One important factor was sheer cost—the need to raise funds in a tight capital market and to build houses at a price that working-class tenants could afford. Among the more intangible factors were the expectations of tenants themselves—their stubborn preference for parlors and passages and for the (officially despised) "semi-detached."

The most fundamental pressure, according to Swenarton, was that of government ideology. The integrationist policies of 1919–20 dictated that working-class housing should be not merely more abundant than before, but also superior in quality. Official goals were spelled out in the Tudor Walters Report of 1918, which prescribed standards for working-class houses comparable and in some respects superior to those of the contemporary middle class. The fruits of this report were seen in the high quality of many council houses built in 1919–20, such as the London City Council's Roehampton estate. Late in 1920, however, boom turned into slump and workers lost their threatening power. The Geddes axe fell, and with it the volume and quality of the housing program. Government policy shifted from high quality to low costs and from local authorities to private contractors.

Swenarton's theme is a fascinating one, and few will quarrel with his view that housing policy in 1919 was fraught with political implications. Certain aspects of his argument, however, are either unsur-

prising or unconvincing. It is scarcely surprising to find that government policy was designed to promote social order; when a historian finds a government deliberately promoting *disorder*, then indeed we shall be forced to sit up and take note. The major significance of this book lies, however, in its discussion of design, and it is here that the argument remains disappointingly unconvincing. Architecture has its political iconography like any other art, but the very fact that two diametrically opposed schools were simultaneously in vogue throws at least some doubt on the claim that design was merely a tool of the state. The fact that middle-class housing styles changed during this period in much the same way as public-sector housing styles suggests that taste and fashion were to a large extent independent of state control. Swenarton's exaggerated emphasis on the role of the state perhaps stems from the concentration of his research on central government institutions. A study that looked more closely at architects, town planners, political parties, and local authorities (here dealt with in one rather sketchy chapter) might yield the kind of insights into competing architectural ideologies recently thrown up by Carl Schorske's *Fin-de-Siècle Vienna*. Such an approach might solve some of the puzzles about the politics of design left unanswered by this book, such as why in 1915 the British government chose to house its munition workers in a stage setting not unworthy of the Meistersingers of Nuremberg. Plausible reasons for such a policy are not hard to guess at, but one would like to root one's conjectures in a more tangible basis of facts.

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JOHN MACNICOL. *The Movement for Family Allowances, 1918–45: A Study in Social Policy Development*. (Studies in Social Policy and Welfare.) London: Heinemann. 1980. Pp. xiii, 243. £15.00.

In his study of *The Movement for Family Allowances, 1918–1945*, John Macnicol deliberately refrains from exploring either such theoretical issues as the purposes or causes of welfare programs or such social and economic themes as the women's movement or the wage system under capitalism. He focuses instead on the interaction of intellectual, economic, social, political (including lobbying), and administrative factors to determine "on what grounds were family allowances acceptable to policy makers" (pp. x, 216). Recognized as desirable by the wartime Stamp Survey in 1939 and the Beveridge Report, their delayed enactment in 1945 provided a modest tax-financed weekly payment to each family for the second and each additional child.

In analyzing the interwar debate over family

allowances, Macnicol concludes that the demographic argument was hollow but that the family poverty appeal had substance. Governmental programs, however, were limited mainly to supplementary unemployment payments (based on family size) originally instituted as a temporary measure in 1918. Whitehall and especially the Treasury resisted expanding this program to include all families until World War II. Macnicol strongly asserts that wartime acceptance of the change was not a response to logical arguments, public opinion, lobbying, or political pressure. Rather, the high civil servants reluctantly accepted it and then "only for reasons of economic control" (p. 217). Initially, it could help the war's anti-inflation campaign by limiting significant income increases to those with large families. In the future (since unemployment benefits must be high enough for subsistence but low enough to encourage employment), it would maintain work incentives, ensure labor mobility, and alleviate some problems of low pay. Thus family allowances would preserve the economic status quo.

Problems exist in Macnicol's major World War II chapter and conclusion, where he boldly makes but often contradicts his main thesis. He acknowledges that public opinion and political pressure by 1943 did force officials to develop a family allowance scheme whose implementation was delayed for two more years (pp. 190–91). By then it was being handled as a postwar reconstruction program. His term "policy makers" refers mainly to high civil servants rather than to ministers, who, as Macnicol states but never demonstrates, played a secondary role (p. 218). Needed are analyses of the underlying tension within the coalition over social reforms during the war, the family allowance issue's relative significance among other reforms, the extent of key Labour party leaders' commitment to family allowances, and the role of pertinent ministers in the controversy. Moreover, since Macnicol declares that high civil servants acted in a biased way on this issue, a more coherent coverage of their role is also needed.

This revised doctoral dissertation is thoroughly researched and heavily documented. Macnicol writes clearly and usually provides adequate introductions to each new event or topic, although oddly the 1945 act itself is never explained well. He deals best with the interwar movement for family allowances, the varied arguments supporting it, and Eleanor Rathbone and the Family Endowment Society's lobbying activities. Macnicol's case study makes a solid contribution to social policy history.

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D. R. THORPE. *The Uncrowned Prime Ministers*. London: Darkhorse Publishing; distributed by Human-

ities Press, Atlantic Highlands, N.J. 1980. Pp. xv, 263. \$20.75.

Austen Chamberlain, Lord Curzon, and Richard Austen Butler held virtually all the important British government posts, but they all failed to achieve the prime ministership. All three sought the highest political prize (Curzon having coveted it since childhood) and were at times considered odds-on favorites, but all were passed over in favor of more obscure candidates. D. R. Thorpe's study sheds new light on the reasons for the trio's failures and in the process enlarges our knowledge of the methods by which the prime minister has been chosen during this century. Also highlighted is the nature of political leadership and, quite significantly, the importance of luck.

Austen Chamberlain's famous father trained him for a career of great public service. His political life, however, unlike his maverick father's, became entirely predictable. Austen was the archetype of the English gentleman, but that gentility became a political handicap, although ultimately it was his fatalistic view of political opportunity and uncertainty as to his own ability that cost him the premiership. Thorpe implicitly concurs with Arthur Balfour's observation that Chamberlain's peers rejected him in 1922 because he was such a bore. Curzon's is the most interesting story. By the age of 36 he already had experienced a rich and exciting life. But he sought more, only to have his bid for the premiership in 1923 undone by his overbearing personality. Temperamentally unable to delegate responsibility, his haughtiness and pettiness cost Curzon the support of his Tory colleagues, if not the general public. R. A. Butler, the third aspirant, has the distinction of nearly having been prime minister twice. He appeared headed for the top in 1957 after an illustrious career in government highlighted by his sponsorship of the wartime Education Act. An emerging anti-Butler faction, however, blocked his path by arguing that Butler's commitment to Tory principles was intellectual and not emotional. Conservatives, after all, love a scrapper as a leader: look no further than Margaret Thatcher. Butler also lacked the sense of determination and the gift of oratory possessed by the victor, Harold Macmillan.

Specialists as well as general readers will find this book worth reading. The author demonstrates that Tory prime ministers and party leaders often have been selected because they are negative choices; that is, they have not alienated important elements within the party. Chamberlain, Curzon, and Butler all had made key enemies. The section on the 1911 intraparty struggle over the leadership is particularly revealing. Thorpe's analysis of the motivations and character of Austen Chamberlain will remain the standard interpretation for years. Few readers

will agree, however, with the suggestion that Chamberlain's reputation needs rehabilitation.

Conversely, the section on Butler is weak and unsatisfactory. Unlike the presentations of Chamberlain and Curzon, Thorpe does not offer a searching critique of Butler's abilities. Members of the anti-Butler faction are not identified, perhaps because sensitivities are still raw, but surely Lord Butler and his friends could not object to such a revelation. Too much extraneous material permeates the text; the sections on Curzon's early life and his restoration of Tattershall mansion are prime examples. And because of the organizational structure, the sections on Curzon and Chamberlain unnecessarily repeat much of the history of the Lloyd George coalition in the early 1920s.

At times Thorpe gropes for similarities among his three subjects. It is of no consequence, for example, that India was important to each one of them. Then, too, his speculation on what kind of prime minister each would have been is of little interest to serious historians. Despite extensive research, Thorpe is able to offer here only the skeleton of a conceptual framework. A thorough study of those who failed to climb to the top of the greasy pole must, of necessity, include more candidates than those treated in this volume.

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PAUL HAGGIE. *Britannia at Bay: The Defence of the British Empire against Japan, 1931-1941*. New York: Clarendon Press of Oxford University Press. 1981. Pp. xii, 264. \$49.50.

Another chronicle of Britain's demise as an eastern power in the thirties? Evidently a fair number of historians are as fond of misery as misery is of company. Enough of them have written on this dreary decade, at any rate, to make the assertion that it is "much neglected" (p. vii) only so much wishful thinking on Paul Haggie's part. Still, we are in need of a fresh analysis dealing with the decade as a whole, one sufficiently thematic to enable us to superimpose some continuity on the recent fractional accounts by Lee, Trotter, Lowe, *et al.* Haggie's theme—how Britain responded to the naval challenges of the period—is overstretched, but certainly has promise. In terms of meeting our current need, however, his discussion comes up short.

The reasons are instructive. The bottom line of the story is that Haggie really has no bottom line. Despite much solid research, chiefly in British cabinet and Admiralty files, he winds up losing his tale in the telling. Much of the problem lies in the pedestrian way in which he handles his material. Rather than trying to explain what his documentary

evidence means, he is content merely to summarize what it says. The result is paraphrase after wearisome paraphrase of official *pronunciamientos*—"ponderous, polysyllabic papers," to quote the author (p. 139)—consisting not just of cabinet minutes, dispatches, and the like, but ten years of innumerable defense plans debated innumerable times by innumerable committees (collectively causing the historian now, one suspects, more confusion than the Japanese then). It is an impressive accumulation of data to be sure, until one asks what the significance is.

On this essential question, Haggie offers little guidance. Obscuring his view as well as ours of the decade's overarching themes is a narrative sliced into chronological segments so self-contained as to be virtually independent. Add to this occasional digressions of questionable relevance (for example, the Invergordon Mutiny in 1931) and frequent forays into the foreign policy area (generally so abbreviated as to make even the well-canvassed Simon-Stimson misunderstanding of 1932 barely intelligible), and the pattern of inconsequentiality becomes, alas, too obvious to be denied.

Such conclusions as the author does draw, for the most part an unexceptional lot, tend to corroborate that he was too passive in asking "why" and, more important at times, "why not" of his material. It is hardly convincing, for example, for him to fault British defense planners for their "lack of dynamism" (p. 211) and "inability to react swiftly and positively" (p. 210), when his own evidence massively shows the actual culprit to have been, in Churchill's words, the "awful enfeeblement" (p. 191) of Britain's imperial power. True, Admiralty staffers did pursue "an obsolete maritime strategy with inadequate resources" (p. 211), but what other course, realistically speaking, does he believe they could have followed?

Haggie's discussion is not without value. It will be especially useful to those seeking detailed insight into Whitehall's thinking concerning defense capabilities in East Asia throughout this time of exceptional torment. For those in search of an interpretative analysis of the thirties, however, the book has undeniable shortcomings. Victor Hugo was right: history is not written under a microscope.

ROBERT J. GOWEN
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WILLIAM L. MILLER. *The End of British Politics? Scots and English Political Behaviour in the Seventies*. New York: Clarendon Press of Oxford University Press. 1981. Pp. xi, 281. \$49.95.

Only rarely has Scottish politics occasioned the degree of excitement and the public attention that is

customary with politics in Northern Ireland. The rare modern exception is the issue of devolution and the dramatic increase in electoral support for the Scottish National party in the 1970s.

Although William L. Miller deals in an early chapter with the background of Scottish politics since 1945 and in a later chapter discusses the inequities of the voting system that in Scotland has favored the Labour party, his book does not have Scottish nationalism or the working of Scottish political institutions as its central focus. Rather, it is mainly a study of political behavior based on a nationwide political survey of which the author was the Scottish director. With its considerable number of tables and diagrams, some mathematical analysis of voting, and a recursive causal model applied to cross-sectional data, the book is hardly entrancing reading, but it is not obscure or rebarbative. Although short on explanation and devoid of insight, *The End of British Politics?* clearly presents a detailed analysis of Scottish and English public opinion in the 1970s, using the data of the British and Scottish election surveys to compare Scottish and English political attitudes across a wide range of variables and to quantify their differences.

As in many works by behavioral political scientists, the interesting and the useful alternate with the obvious and the less than startling. It is interesting that less than half of the survey respondents identified with any class and that of those who did, the Scots were more willing than the English to identify spontaneously with the working class. It is not unexpected that interest in politics was highest in the London area and lowest in Scotland. Scots, naturally, were more concerned with Scottish issues—oil off Scotland and Scottish government—but they also differed from the English on a whole range of other issues. They were far less antagonistic toward colored immigrants, less favorable to the European Economic Community and to controls over pollution, and more concerned with proposals for social services, poverty, redistribution of wealth, and comprehensive schools. All the UK-wide issues were less important to the Scots.

Miller's data show the nature of support for independence, devolution, and the SNP. The SNP attracted those who were preoccupied with Scottish issues, who were disaffected with the two major parties, who were young, Protestant, or irreligious. But perhaps most interesting is the fact that the most important social and partisan influences on voting—class and major party identification—were roughly similar in Scotland and England.

Miller does not answer the question in his title. The 1979 election eclipsed, at least temporarily, the Scottish dimension in politics and belied the premature view that the Labour-Conservative hegemony over British politics had ended. But Miller wisely

concludes that there is still an urgent need for constructive constitutional change in Scotland.

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CHRISTINA BEWLEY. *Muir of Huntershill*. New York: Oxford University Press. 1981. Pp. xi, 212. \$24.95.

Thomas Muir of Huntershill (1765–1799) was one of the handful of genteel Scottish radicals whose enthusiasm for the French Revolution did not diminish after the September Massacres and the storming of the Tuilleries. Muir continued to organize Scottish artisans in Revolution Societies having contacts with those in England, and he agitated for political and ecclesiastical reforms. When the convention of the Delegates of the Scottish Friends of the People met in Edinburgh December 11–13, 1792, he was one of its leading figures. His outspoken support of the Revolution, of the United Irishmen, of Paine, and of a variety of radical causes made him an object of governmental attention. Early in 1793 he went to France, ostensibly to plead for the life of Louis XVII but possibly to avoid prosecution. Returning to Scotland clandestinely on July 30, 1793, he was caught, tried before biased judges and an unsympathetic jury, and sentenced to fourteen years' transportation to Botany Bay. After some time in prison and in the hulks, Muir was shipped to Australia with T. F. Palmer, William Skirving, and Maurice Margarot. Muir escaped from Australia on an American ship bound for what is now Vancouver. From there he made his way south and east across New Spain to Cuba and then was sent to Spain and France where he died early in 1799.

This book is the story of his life and is useful in a number of small ways. It shows that Britain was not without advocates rather like many of the lawyers swept up in the French Revolution—idealistic, articulate, somewhat reckless, and not overly scrupulous. Muir pursued goals that he thought were widely shared but that could not have been realized in England or even in more radical Scotland. The book contains a sketch of radical beliefs, and it shows how thoroughly they frightened the Scottish politicians. In its glimpses of the expatriate British community in Paris in 1793 and later, it perhaps adds a bit to what is known just as it sheds some light on the characters of Muir's fellow martyrs and on the conditions that they endured, both in the hulks and in Botany Bay.

On the whole, however, it is a book that does not make any important additions to our knowledge of anything save the details of Muir's life. The author, Christina Bewley, has used some newly found Spanish, French, and British sources, but her research might be more valuable had she adequately footnot-

ed the book and not prefaced the notes to each chapter with general notes (for example, "Principal sources, minutes, notes, reports and letters in H.O. 102/6–7" [p. 196]). The book's major shortcoming, however, is a multitude of annoying inaccuracies. Those who use it should check the details in the historical background material, or they may be misled into thinking such things as that Scotland had thirty MPs sitting for burghs (p. 17), that Edinburgh's population in 1780 was 160,000 (p. 18), or that Dr. Richard Price invented the sinking fund (p. 22).

ROGER L. EMERSON
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ENID GAULDIE. *The Scottish Country Miller, 1700–1900: A History of Water-Powered Meal Milling in Scotland*. Edinburgh: John Donald Publishers; distributed by Humanities Press, Atlantic Highlands, N.J. 1981. Pp. x, 254. \$37.50.

Given the persistence of the energy crisis, this work of history has timely relevance. Here Enid Gauldie eschews the urban working-class housing that occupied her attention in *Cruel Habitations* and turns to the land to record and to document the life and work of the Scottish country miller. As Gauldie says, the history of Scotland has been too often the history of elites; yet as E. P. Thompson, John Foster, and others have ably demonstrated, the history of the working class is notoriously difficult territory for the historian. One possible way to come to grips with working-class history is to single out a group whose occupation makes it notable and liable to be commented on by contemporaries because of its importance to the community. The miller—himself part of a working-class elite—was vitally important to small-town and village life, and his product was the basis of Scottish diet until the Victorian era and even later in certain rural communities in northeastern Scotland and the Northern Isles. Gauldie skillfully marries conventional source material with oral tradition and industrial archaeology to produce a rural counterpart to the growing number of studies on urban-industrial labor.

Dietary habits that sustained popular demand for natural products do much to explain the country miller's survival long after the introduction of mass-produced foodstuffs, especially in more remote parts. So it is no surprise that much of the book has a regional bias toward the northeast of Scotland, one of the main cereal districts. The subsequent discussion reveals the complex origins of mills, as appendages of abbey, town, or barony and at the same time explains the system of thirlage that bound tenants to it. As long as thirlage survived, landowners had a financial stake in their mills, but contrary to the view stated here, there seems to have been

extensive mill building and renovation even after the passage of the Thirlage Act in 1799, which allowed estates to be freed of thirlage. This wave of mill building was prompted by the profitability of the grain trade during much of the period before the repeal of the Corn Laws.

Gauldie is on safer ground in her chapters on mill buildings, mill technology, and power, but illustrations would certainly have helped those readers unfamiliar even with simple mechanical devices. The later social aspects are altogether stronger, and there is ample proof, as Gauldie says, that the miller was no shifty rogue cheating his customers at every turn but rather the welcoming "jolly miller" of folk tradition.

Sadly, there are some lost opportunities in this otherwise excellent book. No real attempt has been made to examine the economic significance of mills and milling in a wider context, the role of the milling industry in local and regional economic growth, or its relationship to agriculture and other primary processing industries. Even the relationship of the milling industry to the grain trade merits a far more detailed discussion. The book discounts much useful source material for areas other than the northeast, particularly given the importance of milling in southern districts like Lothian, Berwick, Dumfries, and Galloway. Why was no use made of eighteenth- and nineteenth-century insurance valuations (mills presented a big fire hazard) for data on ownership, capital, buildings, and machinery that might have provided the basis for an invaluable national survey of the industry, leading to useful cross-comparisons with other sectors? Surely social history can only benefit from such quantitative insights. Yet these criticisms apart, *The Scottish Country Miller* calls attention to an almost forgotten class and a lost industry once at the very heart of Scottish rural life.

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MICHAEL TIERNEY, *Eoin MacNeill: Scholar and Man of Action, 1867–1945*. Edited by F. X. MARTIN. New York: Clarendon Press of Oxford University Press. 1980. Pp. xxii, 409. \$67.50.

As every Irish schoolboy knows, Eoin MacNeill was the one who issued the famous countermanding order to the Irish Volunteers on Easter Sunday 1916 and thereby made certain that the planned rebellion against British rule in Ireland would be confined essentially to Dublin. For this, the late Michael Tierney, his son-in-law, observes that he suffered thereafter "a strange systematic denigration." Actually, he did far better than might have been expected for a leader who declined the opportunity to enter the Valhalla of Irish nationalist

heroes. MacNeill spent the opening days of the rising in hiding in an Augustinian monastery. He lay low for the rest of that fateful week, emerging only to offer his services to General Maxwell to help prevent any further resistance by Irish Volunteer units lately under his command. Instead, the general clapped him in jail and doubtless saved his career: "We would have no political future if we were not arrested," he had warned Bulmer Hobson (p. 222). After his release, MacNeill was soon back in harness as a member of the Sinn Féin executive before being elevated to MP, speaker of Dail Eireann, and minister in the Free State government. He had even been allowed to resume his chair of history at University College Dublin. His brief career in politics, spanning the years 1913 to 1925, serves as a classic illustration of the problem of the intellectual in politics. The qualities of intellect that gave him access to power reduced his capacity to lead and act in a time of crisis.

MacNeill was a well-known figure and respected academic when he entered the political arena in 1913. He (not Douglas Hyde) founded the Gaelic League and had already acquired an international reputation as a Celtic scholar. The establishment of the anti-home rule Volunteers in Ulster led him to urge a contrary paramilitary response by southern nationalists. This brought him to the attention of the revolutionary elements in Dublin, who were in need of a front man both to lull suspicion and to attract support while their plans for insurrection matured. When he discovered at the last minute that others were taking all this business very seriously, he was appalled, for he had been merely playing at soldier, promising action only if the government moved to take away his guns. But more could not have been expected of him: he had a moralizing approach to violence and rebellion that would have immobilized every would-be patriot and revolutionary from Joan of Arc to Lenin. As he reminded Patrick Pearse in a revealing memorandum (regrettably not included) shortly before the rising, "an armed revolt at present would be wrong and unpatriotic and criminal"; were he to join in, it would involve him in "the guilt of murder."

This biography is an exculpatory tour de force, a work of pietas. Although it gives us some interesting sidelights on MacNeill's dilemma in 1916, it adds little of great import that is not already dealt with in an earlier study (F. X. Martin *et al.*, editors, *The Scholar Revolutionary* [1973]), a more comprehensive work in many ways and certainly more balanced. The MacNeill described in the book reviewed here is entirely without flaw: he is, in the author's words, a rocklike character of tireless energy, more revolutionary than the revolutionaries, one of Ireland's greatest leaders, and more. Yet contemporaries, even friends, spoke of his temperamental indolence,

faulty judgment, and unfitness to lead. Not surprisingly, he is fully absolved of the least tinge of incapacity or misjudgment in his role as boundary commissioner in 1924–25, an ill-fated episode that brought humiliation to his government and caused his own political demise. Tierney, who obviously shared MacNeill's contempt for the romantic notion of the blood sacrifice, deals severely with Pearse and his executed comrades while aiming sledgehammer blows at what he terms the "foundation legend" or "1916 myth." Indeed, he may be the first Irishman in over seventy years to find merit in the Irish Council Bill of 1907 and to suggest speciously that its adoption would have saved Ireland and Great Britain from "multiple disasters." Observations that do not square with the author's speculations lack "high evidential value," a charge that could fairly be leveled on occasion at MacNeill's unpublished memoirs, dictated many years after the events described and forming much of the basic source material for the work. The bibliography also disappoints, there being no apparent attempt to go much beyond MacNeill's own private papers and no scouring of the archives in Dublin, New York, or elsewhere for his letters to leading figures of the day. (As his letters to Roger Casement show, MacNeill had a lively prose style.) One may also ask what is happening at the Clarendon Press, for there are over a score of errors (mostly misspellings) in addition to wrongly spelled names, wrong dates, and, among other slips, references to an Austro-Hungarian parliament of 1861 and a Home Rule bill that was introduced in 1911.

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ROBERT J. SEALY. *The Palace Academy of Henry III*. (Travaux d'Humanisme et Renaissance, number 184.) Geneva: Librairie Droz. 1981. Pp. x, 209.

"The question of what happened to the academic movement under Henri III is one of some perplexity." This admission by Frances Yates did not deter her, in her classic study of French academies, from placing the Palace Academy of Henry III firmly within the framework of sixteenth-century academism. Building upon the studies of Pierre Champion and the much earlier work of Guillaume Colletet, Yates described the Palace Academy as an integral part of a movement that had its roots in the syncretism of the Platonic Academy of Florence. She saw it as a branch of Baïf's Academy of Poetry and Music, an extension that balanced the Platonism of the latter with lectures on Aristotelian ethical categories and natural philosophy, completing what Baïf envisaged as the encyclopedia of knowledge. On the religious plane it represented a stage in the

moderate line of Counter Reformation spiritual and moral reform leading from Charles Borromeo's own academy, the *Notti vaticane*, its model and inspiration, to Henry III's *Congrégation de Notre Dame* at Vincennes, where the "Philosopher-King" became the "Penitent and Hieronymite King."

Sealy's book is a thorough assault on Yates's thesis, an attack carried out with sharpshooting precision rather than grenadelike explosion. Utilizing evidence already known, Sealy has meticulously reconstructed a virtual day-to-day account of the meetings of the academy. Beginning early in 1576 as biweekly lectures in the Louvre, these changed into daily dinnertime disquisitions as the orators followed Henry III back and forth from Paris to the provinces for three and one-half years. According to Sealy, the Palace Academy was conceived neither as an attempt to tap into the intellectual currents of the period nor as a means of effecting spiritual reform, and there is no evidence to suggest that it was intended as a continuation of Baïf's academy. Its more prosaic purpose was to instruct the king and complete his faulty education as painlessly as possible. This explains its personnel, its *modus operandi*, and its subject matter, all of which centered on the needs of Henry III. The king was its prime mover (not Pibrac, as Frémy claims); he dictated the subjects of the lectures, and his partial deafness marked their termination. Unique as it was, the Palace Academy nevertheless embodied the main intellectual trends of the decade; as its lectures proceeded from Aristotelian moral philosophy to natural philosophy, the author concludes, it served as "a stage in the transition from humanism to scientism" (p. 173).

If one overlooks the glaring deficiencies of the book—frequent irrelevancies, labored arguments over inconsequential, undigested quotations, text that should have been footnotes—Sealy's revisionist argument is quite compelling. Yates's thesis will undoubtedly have to be altered. For one thing, the influence of Borromeo on Henry III's academy has been shown to be unlikely. Even the portrait of the king is given different form: the monarch Yates depicts as a religious reformer urging moderation is here seen as a would-be philosopher-king aware of his ignorance and anxious to overcome it. As for the academic movement as a whole, having cast doubt on the accuracy of one conception, Sealy offers no alternative vision.

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PAUL LAWRENCE ROSE. *Bodin and the Great God of Nature: The Moral and Religious Universe of a Judaizer*. (Travaux d'Humanisme et Renaissance, number 179.) Geneva: Librairie Droz. 1980. Pp. xvii, 237.

Jean Bodin has not been the subject of an adequate intellectual biography. Much of his work, particularly the later writings, is complex and obscure: a labyrinth from which it is difficult to emerge with any idea of overall pattern. Paul Lawrence Rose feels that he has found this pattern in Bodin's religion, a set of beliefs that, when understood, permits a coherent interpretation of ideas and actions heretofore regarded as puzzling and inconsistent. He argues that Bodin was not a Christian but rather a Judaizer, one who adopted a Judaic ethic and cosmology but not specifically the Jewish religion. His beliefs avoided the modern distinction between theistic and natural. God established the universe and its laws, intervening by means of angels and demons to protect and to punish. Natural and divine law are basically the same. God grants knowledge of this law, and man, through free will, has the ability to follow it. In so doing, he becomes purified. Some who have attained this state receive the gift of illumination and become prophets, recalling others to the law from which they occasionally drift.

Rose finds elements of these ideas in Bodin's early writings and argues that by 1566 he was definitely "converted." Finally, during the period 1566–69 he became a prophet, and some of his subsequent actions, such as his apparent wavering at the time of the League, may be regarded as stemming from this aspect of his religious convictions.

Rose's analysis is certainly a contribution to Bodin scholarship, which has largely ignored the religious aspect of Bodin's thinking. On the whole, the arguments are convincing, especially when applied to the later writings. There is, however, a potential methodological problem. Rose's findings, prolegomena to a future intellectual biography, are presented topically and are largely divorced from the biographical and even intellectual context in which Bodin's thought undoubtedly developed. There is thus a tendency to infer Bodin's earlier ideas from what is known about his mature thought, the working assumption apparently being that there exists continuity between the two. This may well be the only way of handling the matter, but short of further elaboration the results must remain tentative.

This line of thinking also poses another fundamental question relating to Rose's future efforts. Can religion, as he maintains, be used as a basis for understanding Bodin's thought and actions? While it is undoubtedly true that sixteenth-century people regarded their experience differently from those of the twentieth, were they any more consistent in following their world view than their descendants are in following theirs? Bodin's Judaizing was undoubtedly important, but to regard it as a master key seems somewhat reductionistic.

In spite of these caveats, Rose has produced a useful volume. Its ultimate value, however, will be determined by how he is able to employ its analysis in his future work.

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EMMANUEL LE ROY LADURIE. *L'argent, l'amour, et la mort en pays d'oc; Précédé du roman de l'abbé Fabre, Jean-l'ont-pris (1756)*. (L'Univers Historique.) Paris: Éditions du Seuil. 1980. Pp. 585.

Almost twenty years ago, Emmanuel Le Roy Ladurie published *Les Paysans de Languedoc* (1966). In that first major work, Le Roy Ladurie established himself as a historical demographer, human geographer, price historian, rural sociologist, and expert on "social structure." Equipped with a prodigious variety of archival sources "in series," mobilized behind a *longue durée* of three centuries of agrarian history, and possessed of an unmistakable passion for Mediterranean Languedoc, Le Roy Ladurie wrote *Annales* history at its best. Now, six major books later, the distinguished professor of the Collège de France writes with disarming modesty: "One will therefore forgive the rural *chercheur* that I am for attacking a problem of popular culture whose contours do not require the encyclopedic knowledge of a historian of religion who reads forty languages, nor the genius of one of the greatest anthropologists of our time" (p. 453). "Attack," indeed, for Le Roy Ladurie now becomes literary *structuraliste* and comparative folklorist in order to explore—*globalement et en détail*—the origins and structure of a single novella written in Occitan and published in 1756 by one Abbé Fabre, "one of the greatest men of letters in the land of Oc" (p. 67). The purpose of this meticulous, indeed exhaustive, exegesis of Fabre's text is not only to establish the distant origins (in time and space) of this *conte*, but also to demonstrate that Fabre's *Jean-l'ont-pris* is the key to the normative, prescriptive fiction and theater of the Midi in the seventeenth and eighteenth centuries. Fabre is at the crossroads of two cultures, written and oral, learned and popular, secular and magical. The abbé emerges as both a Balzac and a Grimm, a literary artist who adapts the model of a multinational folktale, "Godfather Death," known in over three hundred fifty variations since 1350, and the repository of a rich oral tradition that contains—although Le Roy Ladurie does not use Jacob Grimm's phrase—the "soul of the common people."

If Le Roy Ladurie's book begins with a design or model for the infinite variety of marriage strategies employed in a province of exceptionally strong paternal power (reflected in the whole extant corpus of Occitan literature), it moves soon enough into an extended textual analysis of Fabre's tale for more

than three hundred pages and forty tables. Although a bona fide *structuraliste* like René Girard or Louis Marin should make the final assessment here, there is no doubt that Le Roy Ladurie is familiar with a whole range of *structuraliste* techniques appropriate for establishing those "remarkable concordances" between Fabre's text of 1756 and the late medieval folktale *La Mort-Parrain*, especially in its Swiss-German, Catalan, and provincial French versions. Le Roy Ladurie moves on every level, from the surface parallels of names, episodes, and themes to the deeper correspondences "decoded" from the tone, quality, or symbolic meanings of an action or a textual juxtaposition, demonstrating his virtuosity at detecting, diagramming, and interpreting inversions and double entendres, chains of signifiers, and disguised symmetry.

The professional apparatus of literary criticism and comparative folklore is happily cloaked in Le Roy Ladurie's playful style, peppered with North American idiom—"happy end," "best-seller," "re-make"—and a rich mixture of Rabelaisian vigor and *Annaliste* imagery—*écoulements*, *double glissements*, and tips of icebergs. But beneath the surface, this reader has the distinct impression that he is engaged in a game with Death, not in the earnest solemnity of a Bergman film, but in the roughhewn, physical, violent world of sun-baked Mediterranean villages. The game is not chess, but the shrewd ploys of peasant cunning—ruse, bluff, chicanery, dupery—all in an effort to outwit the powers of Death and, *mutatis mutandis*, analogous to the machinations of the hero to gain the consent of the girl's father. All this is a part of a fundamental rite of passage to male adulthood and marriage played out "in the macabre stages of a necessary initiation" (p. 454). In the end we are back to the eternal verities of the villages of Lower Languedoc—ambition and bridewealth, love and death.

Has Le Roy Ladurie deserted the empirical world of stone villages and tangible vines for multiple meanings and "remarkable correspondences"? Not at all. There is not one structure but many, each with its configuration and appropriate set of tools. "The diffusion of themes studied here," he writes, "does not pose the vast problems that are implied by a cultural transfer from the Himalayas to Sweden" (p. 451). If Le Roy Ladurie does not posit a universal and timeless mental structure, he does believe that his extensive foray into the "*socio-littéraire*" has uncovered a *longue durée* of oral literature extending over five centuries and transcending national boundaries. His exhaustive analysis of Fabre's tale and its antecedents has identified two levels of "infraculture" in the eighteenth century, a ground floor of secular provincial writers and "a *sous-sol* which is crisscrossed by cables of an oral network for the transmission of the marvelous" (p. 508). But

even in 1756 Godfather-Death is nearing the twilight of his long journey over a half-millennium. Alas, the puritanic, centralizing, class-conscious nineteenth century is close upon us. The Western world is disenchanted.

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JEAN-PIERRE GUTTON. *Domestiques et serviteurs dans la France de l'ancien régime*. (Aubier Collection Historique.) Paris: Aubier Montaigne. 1981. Pp. 252.

Domestiques et serviteurs dans la France de l'ancien régime by Jean-Pierre Gutton addresses a topic that is important but difficult to pursue because of the scarcity of evidence. The author, however, has been very imaginative in using extant materials like wills, inventories, court records, the text of laws, and all kinds of literary works, including plays and prose exposés on servants in general. He has done a considerable amount of research in the area around Lyons. Nothing has been written directly on servants since the late nineteenth century.

This is a useful book for anyone who seeks to know about a numerous group of people whose exact definition is difficult to give. Some servants worked for important people, including the king, and were, therefore, very influential. Others worked for farmers and had little status. Some were given board, room, and clothes, and others earned regular wages. Gutton examines the *domestiques* first, mainly those employed by aristocrats and royalty but not usually receiving a set wage. He stresses the importance of the medieval idea of fidelity between master and servant that continued throughout the Old Regime. The relationship went both ways, with the servant responsible for fulfilling his duties and the master responsible for the spiritual life of his employee as well as his protection and well-being, even in old age. The number of servants employed depended on the functions of the master. Normally, servants were lodged in the house, but in the eighteenth century that was not always the case.

Gutton next examines the *serviteur* who served the middle classes. Men were on the whole socially and economically higher in this group, but women were more numerous. While the *domestiques* frequently came from the country, even more apparent were the rural origins of the *serviteurs*. Although those kinds of servants were often more like twentieth-century workers, they were still "fort dépendants de systèmes de clientèle et de fidélité" (p. 99).

The agricultural servants were more clearly among those who were paid wages; but, as Gutton notes, they are the most difficult to investigate because not only these servants but also those for whom they worked were illiterate. Gutton concludes that the agricultural servants had several of the

same characteristics as the other kinds of servants: their condition depended on their master; there were several ranks within their social order; and, finally, although they were paid set wages, they were still part of the household.

One of the concluding chapters attempts to establish the larger social context by discussing the laws pertaining to servants. Gutton finds that they were seen by society as possible vagabonds and, thus, potentially a social problem. An analysis of literary sources turns up the same "image pessimiste" (p. 157).

In the final chapter, Gutton examines the servants' ambiguous position. They came from the lower groups in society; yet sometimes they were associated with the higher groups because of their occupation. Through an analysis of servants' inventories, Gutton discloses that some of their possessions were like those of their employers. He also explores the role of servants in the courts, especially with respect to sexual behavior and domestic thefts. In these contexts, one must remember that the extant documents give only part of the picture. For example, court records may indicate that food was seldom stolen by servants, although this was probably not really the case. It was much harder to detect a servant who had stolen food in his own household than to detect an outsider who had taken food, since the servant usually had easy access.

At the end of his book, Gutton suggests that the links between the family and its hired servants need further research. Probably the development of the conjugal family together with the development of the idea of intimacy had an effect on the number of servants employed as well as on the relationship between master and servant. Servants began to be lodged elsewhere than in the home. That would lead to a more impersonal relationship in itself. Gutton goes on to suggest that the history of servants needs to be seen in the light of the history of town versus country. Towns seem to have dictated to the countryside; to what extent did that really affect the countryside? Although more could be done with local evidence, Gutton's book with its useful appendixes of transcribed documents is a fine contribution to the social history of the Old Regime.

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RODERICK PHILLIPS. *Family Breakdown in Late Eighteenth-Century France: Divorces in Rouen, 1792-1803*. New York: Clarendon Press of Oxford University Press. 1980. Pp. viii, 244. \$55.00.

The subject of divorce in French history has given rise to almost as much controversy and partisan scholarship as the disputed role of the bourgeoisie

in the coming of the French Revolution. The advocates of divorce, both during the Revolution and subsequently, saw it as an example of "liberty breaking the chains of despotism," to use an eighteenth-century phrase. Speaking for the Catholic, conservative, corporatist side of French opinion, writers from de Bonald and Barruel to Taine and Olivier-Martin denounced divorce as the product of irreligion, immorality, and an effort to undermine the most critical unit of society, the family. Furthermore, most earlier studies of marriage and divorce law—primarily *thèses de droit*—were shallow, focusing on the texts of the various laws rather than their implementation and effects.

Roderick Phillips's book has avoided the twin dangers of partisanship and superficiality. It studies the application of the divorce law of 1792 in a particular context: the city of Rouen. The book's introduction and first chapter summarize eighteenth-century law and thought concerning marriage and divorce and describe the family court, a novel institution that provided private arbitration by family members, friends, or more frequently lawyers hired to represent the parties. Subsequently Phillips discusses the number of divorces; the kinds of persons who sought divorce; and the effect of children, property, relatives, and neighbors on the process of marriage dissolution. Divorce in eighteenth-century France was an urban phenomenon and one pursued more frequently by women than by men. Unlike their country sisters, urban women could seek divorce because the city offered them employment and lodging; Rouen's textile industry, especially, was their means to independence.

Several contrasts emerge between eighteenth-century Rouen and twentieth-century America. Persons seeking divorce had not married in their youth; the mean age at marriage of divorced men and women was 29.5 and 26.5, respectively. The presence of children seemed to reduce the incidence of divorce, whether for reasons of sentiment or, more probably, because of economic factors. When a marriage broke down, it was almost always the wife who abandoned the parties' home and sought lodging elsewhere. Indeed, in many cases women were thrown into the street and had to sue to recover their clothing and household goods.

While divorce introduced an element of legal equality between the sexes, the women who initiated divorce suits often had great difficulty in securing their rights. Severe physical abuse, a distortion of the husband's marital authority, was common. Male neighbors were reluctant to intervene in cases of family violence, but neighborhood women seem frequently to have tried to aid one another by coming in groups to the site of domestic strife and assisting its female victims. Phillips emphasizes that this "female solidarity" was based on the neighbor-

hood and not necessarily on ties among kin or coworkers.

In his conclusion, Phillips notes the apparent lack of influence of two elements commonly associated with marriage and divorce: religion and romance. Although the Roman Catholic Church condemned divorce, constitutional clerics accommodated themselves to it, perhaps because of the apparently low incidence of remarriage. The church's prohibition seems not to have bothered those who sought divorce. As for romance, revolutionary rhetoric notwithstanding, there is no evidence that individuals were divorcing their spouses to seek new love in a second marriage. If sentiment provided a means of reconciliation, the records do not show it. The one documented reconciliation that Phillips shares with us involved a couple who gave a neighborhood party to celebrate their reunion, fell to quarreling during the meal, and subsequently divorced!

Phillips's book makes one hungry to know more about the lives of these Rouenais and Rouenaises. Did divorced women recover their property? If they were awarded *pensions alimentaires*, were these paid? What institution enforced them? Did possession of significant property by one or both spouses tend to discourage divorce? And what about the relationship of the new marriage and divorce laws to those governing minority, adoption, paternal power, and inheritance? These questions aside, Phillips has written a provocative volume about an important topic.

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CHARLES B. PAUL. *Science and Immortality: The Éloges of the Paris Academy of Sciences, 1699–1791*. Berkeley and Los Angeles: University of California Press. 1980. Pp. x, 202. \$19.50.

Modern historians have difficulty understanding how philosophers of the Enlightenment could both praise the objectivity of modern science and at the same time regard it as a source of moral virtue. The scientific method as it had developed in the seventeenth century was descriptive, not prescriptive, and there seemed to be no way to extract from it a scientific ethic. Charles B. Paul's study of the *éloges* of deceased members of the Paris Academy of Sciences shows one way in which moral virtue was assigned to scientific activity during the Enlightenment.

The *éloges* were not required by the bylaws of the Academy but were a natural consequence of the interest in rhetoric that had revived during the Renaissance. The *Académie Française* had always had *éloges* delivered in the traditional form of the panegyric described by Quintilian. Bernard Bovier de Fontenelle, who began the practice of delivering *éloges* at the Academy of Sciences, followed a new

form originated by Charles Perrault called the *éloge historique*.

Fontenelle broke with the classical form but retained the goal of the panegyric, which was to bestow immortality on the subject. He was a practiced artist of the *eclogue* or pastoral poem, which he merged with the Plutarchean ideal of civic duty to create a new convention of scientific virtue. The scientist followed the pastoral ideals of simplicity, humility, and austerity in his disinterested search for truth. Fontenelle's *éloges* were written to win the public over to science and to establish science as a positive good. The next two secretaries, J. J. Dortous de Mairan and J. P. Grandjean de Fouchy, followed Fontenelle's lead, but without his wit and literary skill. The Marquis de Condorcet, the fourth and last secretary of the Academy during the eighteenth century, made the *éloges* into vehicles for presenting his social and political views. Condorcet's stance of moral rectitude and his passion for humanity were quite different from Fontenelle's easy wit and graceful irony. Both secretaries, however, identified the pursuit of science with the pursuit of virtue, and the ideology of science that they fostered defined the position of natural science in Enlightenment philosophy.

Paul also uses the *éloges* to provide a brief prosopographical analysis of the Academy, and to illustrate the scientific controversies over attraction and generation that took place between its members. These chapters on scientific controversies are less successful, because the *éloges*, while informative, do not give the full story, and there is not enough space in the book to treat the controversies in the detail that they deserve.

The book closes with six appendixes listing the public *éloges* by date, with pertinent information about them; the unofficial *éloges* written by Condorcet; the *éloges* by subjects; the members *not* eulogized; the sources of the *éloges*; and, finally, three sample *éloges* by Fontenelle, Fouchy, and Condorcet. For anyone interested in the popularization and ideology of science during the Enlightenment, this is an important source.

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ROBERT FOX and GEORGE WEISZ, editors. *The Organization of Science and Technology in France, 1808–1914*. New York: Cambridge University Press or Éditions de la Maison des Sciences de l'Homme, Paris. 1980. Pp. x, 355. \$37.50.

According to a long-standing and popular historical thesis, after a golden age in the early nineteenth century, French science and technology entered a period of decline that eventually resulted in that

country's scientific and industrial humiliation by Germany at the end of the century. Yet, according to the ten papers in this volume (prepared under the auspices of Parex, an international network of scholars concerned with the social relations of science), French educational and research institutions were remarkably vital and adaptive during this period.

The French university, preoccupied with examining lycée graduates and training teachers, seemed an unlikely place for research. Yet, for complex reasons having to do with industrialization, modernization, and the rising professional aspirations of academics, around 1860 the faculties of science began to emphasize research and to show a growing concern for applied science. Victor Karady amasses statistics demonstrating the increase in research-based careers. Craig Zwerling argues that the *École Normale Supérieure* pioneered this shift in career patterns and notes concomitant social changes in the student body. George Weisz links the research ethic to reform in the conservative medical faculties, while Harry Paul shows how the science faculties adapted to the industrial age by incorporating applied science institutes. Moreover, French academics, according to Paul, saw no contradiction in the simultaneous pursuit of pure and applied research.

Throughout the nineteenth century, French industry was well served by a highly responsive system of technical education. Terry Shinn deals with the rise of industrial engineers and their elite *École Centrale des Arts et Manufactures*. But industry recruited more of its technical manpower from the humbler *Écoles d'Arts et Métiers*, considered in the paper of C. R. Day. Educational institutions are the main business of this volume. Space limitations do not allow me to discuss in detail the papers that deal with specialized institutions of research: the *Musée d'Histoire Naturelle*, discussed by Camille Limoges; the prize system of the Academy of Science, by Elisabeth Crawford; the provincial and disciplinary societies, by Robert Fox. All showed the effects of the enhanced research climate.

In the only comparative paper of the volume, Peter Lundgreen concludes that by the end of the century the French system of research and education was structurally very similar to the German. What, then, of the argument for decline?

The institutional studies in this volume unfortunately say little about the quality and nature of research produced by the French system. While doubtless this was a conscious omission, an important dimension of the argument is missing. Disciplinary comparisons are also lacking. Fortunately, the splendid introduction by Fox and Weisz recognizes these limitations and raises larger issues. Institutional and other evidence, they argue, suggests that French science may not have been in decline but

simply headed in other directions. But that is an agenda for a future Parex volume.

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MICHEL L. MARTIN. *Warriors to Managers: The French Military Establishment since 1945*. Chapel Hill: University of North Carolina Press. 1981. Pp. xxii, 424. \$29.00.

The French military has not appealed as a field of study to sociologists and political scientists in recent years. While the colonial wars of Indochina and Algeria lead to a few studies of civil-military relations, the past twenty years have witnessed the almost total absence of any significant work on the military in France. The academic neglect was a natural consequence of a decline in the political and strategic importance of the military. It was only when the military became, in the early 1960s, once again a critical instrument of foreign policy that it emerged from its obscurity. Its strategic role has now begun to attract considerable attention.

Michel L. Martin's study of the French military establishment is the work of a sociologist, and it is inspired by the tradition of military sociology that has long been associated with Morris Janowitz. Martin's book fills a large gap in our knowledge, for it is the first to attempt to assess the changing institutional structure of the military and the impact of societal and technological changes on its different branches.

As one would expect from a political sociologist, Martin gives a detailed view of the social composition of the military, changing recruitment patterns, changing attitudes within the military toward discipline and authority, and the ways in which a military career is viewed. But this mass of information would not by itself be sufficient to enable us to understand the role of the military in contemporary French society. Happily, it is supplemented by a more "political" analysis. Martin's sensitivity to the political framework adds a crucial dimension to his investigation and makes this a first-rate study in *political* sociology.

Indeed, the study opens with a discussion of French foreign policy, thus establishing a framework within which the influence of politics on the military can be understood. Martin argues that French foreign policy after World War II assigned a primordial role to France's overseas empire. As a result, at a time of vast transformations in the sphere of military technology the French military establishment failed to modernize. In the past two decades, however, the military has undergone a profound transformation, due almost entirely to a foreign policy decision taken shortly after de Gaulle became president. The development of France's

nuclear capability—the *force de frappe*—forced a technological revolution in the military. But, as Martin emphasizes, technological modernization did not affect all branches of the armed forces to the same extent. The development of a nuclear capacity affected the navy and air force to a far greater degree than it did the army. And the different exposure by the three branches to the onslaught of modernization explains what Martin calls the “dualistic” nature of the French military. The social, organizational, and professional homogeneity that characterized all branches of the armed forces has given way to a dualism that was wholly absent in the days when the military had only a colonial role.

In a brief review one can only give the gist of what is a rich and solid study: well documented, clearly written, and placed within an important theoretical framework.

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JEAN-NOËL JEANNENEY. *L'argent caché: Milieux d'affaires et pouvoirs politiques dans la France du XX^e siècle*. Paris: Fayard. 1981. Pp. 364.

This is a book in two parts, a seventy-five page essay in print for the first time and ten articles, all but one published before in scholarly journals or the press. The essay makes a case for determining the “weight” of the captains of banking, commerce, and industry—the *milieux d'affaires*—on the “political destinies of the country” (p. 7). We are easily convinced. Jean-Noël Jeanneney explains the difficulties of research (in the main the refusal of French businessmen to permit scholars to burrow through their archives) and hints at the treasure to be unearthed. The articles are the evidence.

The argument is familiar. Jeanneney has scaled the *mur d'argent* and tangled with the “200 families” and the “bourgeois dynasties” in his massive and judicious study of steelmaker François de Wendel (*François de Wendel en République: L'argent et le pouvoir, 1914–1940* [1976]) and in his political primer, *Leçon d'histoire pour une gauche au pouvoir: La faillite du Cartel, 1924–1926* (1977). At times he argues that more than money motivated businessmen (patriotism, for example); at times he argues that profit was more important than anything else. In this book he suggests that, although business money counted for something in French politics, it was not as important as one might think and rarely had the decisive influence that the sloganeers would have us believe.

More public careers were hurt than helped by the tie to businessmen and bankers. Finance Minister Joseph Caillaux was hounded by the opposition because of his friends at the Bourse until his distraught wife shot and killed the editor of *Le Figaro*. Philippe Berthelot, secretary-general of the Quai

d'Orsay, lost his job for using his influence to shore up his brother's faltering *Banque Industrielle de Chine*. Few magnates (such as Maurice de Rothschild and François de Wendel) ever made it big in the *Palais Bourbon* or the *Palais du Luxembourg* even though their names and their wallets may have helped them get in the door.

What of businessmen as a group, their political preferences, and the pressure exerted for or against governments of the right and left? In essays on the *Synarchie*, the technocrats' club that was said to have jockeyed for power at Vichy, and the *Commission de Représentation Patronale*, set up to provide the post-war Gaullist government with a link to the men of commerce and industry, Jeanneney describes a business community without real leadership, unity of views, or discipline for collective action. Recounting the speculation against the franc in 1924, he makes the same point. What unified these speculators was not profession, nationality, or politics, but the chance to make money amid the disarray of French finances. Punching holes in conspiracy theories permits a more reasoned analysis of France's past financial woes and, as Jeanneney has indicated elsewhere, might help chart a course for future governments.

It would be silly and misleading to conclude that fear itself was the only thing that French governments had to worry about. Even disorganized and leaderless “communities” of businessmen could stand together whenever they perceived a social danger. Yet Jeanneney maintains that governments usually overreacted to the threat, distributing “secret funds” to the financial press and political groups to counter presumed business influence or to firm up their allegiance to the parties in power. When Premier André Tardieu asked François de Wendel to bankroll the impoverished right-wing *Fédération Républicaine*, de Wendel was stunned. The *Comité des Forges* might contribute to a government's election war chest but would never approve a contribution to a political organization. Only the government could do that. It did.

Jeanneney beckons us to a field where business, economic, political, and diplomatic history intersect, where conclusions still remain tentative, and where American scholars have already made a remarkable contribution. As historian and publicist, he has helped clarify the connections between business and politics in twentieth-century France.

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RICHARD L. KAGAN. *Lawsuits and Litigants in Castile, 1500–1700*. Chapel Hill: University of North Carolina Press. 1981. Pp. xxiv, 274. \$23.00.

The marked increase in formal civil litigation in sixteenth-century Castile is the subject of Richard L. Kagan's *Lawsuits and Litigants in Castile, 1500–1700*. This impressive study contributes to social and institutional history as well as to legal and Spanish history. It is based on extensive archival research that is meticulously footnoted. Of special value is the appendix on judicial archives.

Seeking to present legal history as a chapter in social and political history, the author faces many problems common to social historians. How does one discuss a general development, for example, that requires close examination at local levels? Kagan attempts to solve this problem by using the *chancillería* of Valladolid and the lower court of the *fiel del juzgado* of Toledo as examples of legal developments all over Castile. He discusses archival material for these two courts, but he does not explain how they are typical for a country well known for its particularism.

The author attempts to avoid writing an elitist legal history by presenting the social context of legal development in Castile. He piques our interest with colorful proverbs and satirical literature, but he does not develop popular attitudes toward formal law. He shows that litigation among family members increased substantially, yet he does not explain why families turned to formal courts to settle their internal disputes. Although the author points out that people from all classes engaged in lawsuits, he shows neither who was willing to become attorneys for the poor nor how the poor found legal representation. Kagan suggests very provocatively that the courts acted as both school and theater for illiterate people, but he does not explain how the poor had access to courts as either spectators or participants. His study becomes a history of the development of a legal establishment rather than a true social history.

Kagan deals frankly with the problems of evidence. He acknowledges the paucity of judicial records for many courts, and he discusses the problems of distortion that may arise because the major sources for the cases before the *chancillería* of Valladolid are executive writs issued for a small proportion of the lawsuits that came to this court. One might ask why he based his statistical tables on a sample of these executive writs rather than the total number and why he did not statistically analyze all the cases brought before the lower court.

This book raises questions that will stimulate further work. It reminds us that formal legal systems grow out of informal traditional systems for settling differences, even though it does not thoroughly explain the relationship between these two systems. The author recognizes the political implications of this formal legal development when he suggests that peasants used the courts of sixteenth-century Castile to challenge the power of their lords.

The question of why peasants chose this strategy at this particular time remains to be answered.

Kagan's study enriches our profession, and we may look forward to more work of the same high academic quality that will address these significant issues in social history.

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JOHN B. OWENS. *Rebellion, monarquía y oligarquía: Murciana en la época de Carlos V*. Murcia: Secretariado de Publicaciones, Universidad de Murcia. 1980. Pp. 296.

With the publication of this book on Murcia, John B. Owens has added to the growing list of specialized studies on Spanish towns and regions in the early modern period. The works recently published by Spanish and foreign scholars, and other works in preparation, will provide us with a much more complete view of the broad outlines as well as the intricacies of Spain's early modern development.

Murcia, in southeastern Castile, was still a frontier city in the sixteenth century and faced the constant threat of attack by Muslim sea raiders. As a consequence, all citizens were obliged to bear arms. This was a potentially explosive situation, since Murcia was dominated by a factionalized and violence-prone elite. Using as his principal sources the minutes of the municipal council, Owens has focused on the inner conflicts of that elite and its relations with the aristocracy and the crown during the time of Charles V. The most potent upheaval in the reign was the rebellion of the Comuneros in 1520–21.

The rebellion is the centerpiece of Owens's study, but he has not confined himself solely to its events. Instead, he describes the background, course, and long-term consequences of Murcia's participation in the only widespread Castilian rebellion in the Habsburg period. He begins with a description of Murcia's government in the early sixteenth century and then examines a host of local concerns that helped lay the groundwork for the revolt: problems of public order, the methods of assessing and collecting taxes, and the activities of the Inquisition, to name some of the most important. As happened elsewhere in Castile, local affairs were the keys to the unfolding of the revolt.

Owens's major theme, analyzed with considerable sophistication, is the shifting relations between the local elite, the crown, and the regional aristocracy. His contention is that the period of the Comuneros and the succeeding twenty years was vital for a reformation of the roles and attitudes of all three groups. There were no real winners or losers, for each group worked out accommodations with the others. The urban elite remained influential and

tended to seek closer ties with the central government. This enabled the crown's intrusion into local affairs to become more complete, since many town councilors welcomed a strengthening of royal justice at the local level. When the aristocracy, exemplified in Murcia by the Fajardo family, found its participation in local government curtailed, it was compensated by gaining responsible posts in the royal government.

Owens has done a thorough job of research and interpretation of the local records, and he exhibits an impressive familiarity with the secondary works on the revolt. One might wish for more comparisons with the progress and consequences of the revolt in other Comunero cities, but perhaps the University of Murcia, which published the book, encouraged a strictly local focus. Spanish historians will welcome this study, because it offers a detailed local analysis useful for testing the broader interpretations of the rebellion of the Comuneros.

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HENRY KAMEN. *Spain in the Later Seventeenth Century, 1665–1700*. New York: Longman. 1980. Pp. xiii, 418. \$44.00.

"A Spain We Do Not Know." With these words, the title of this book's opening chapter, Henry Kamen begins his history of Spain in the age of Charles II (1665–1700). Kamen, of course, is right to refer to our ignorance about this era of Spanish history, for other than the works of Pierre Vilar, James Casey, Angel García Sanz, and a handful of other scholars, historians have been reluctant to delve deeply into a period traditionally associated with decadence and decline. Kamen, author of such books as *The Spanish Inquisition* (1965), *The War of Succession in Spain* (1969), and *The Iron Century* (1971), is therefore to be congratulated for having the courage to attempt a general survey of what is almost uncharted terrain.

With little in the way of secondary literature to guide him, Kamen has been forced to write much of his history from scratch. His sources are primarily those of the epoch, and much of his information comes directly from Spain's archives, both local and national, where the author has obviously spent considerable time. Such diligence has its rewards. Kamen has come up with an astonishing array of new facts and figures, material that is used to paint a grand portrait of Spain in the later seventeenth century. In fact, with the exception of the church, a curious omission, this portrait is almost complete, with individual sections devoted to government, political life, the economy, population, class structure, and education. There is even an attempt to portray culture and art. So large is this canvas that the author has had occasional difficulty finding the

material necessary to cover it completely, but the overall effect is still impressive. Kamen in this respect has succeeded in providing us with the first comprehensive view of this important but sadly neglected chapter of the Spanish past.

The organizing theme of this portrait is that of regeneration. Kamen believes that the commonplace view of Spain in the later seventeenth century as a kingdom in decline is wrong and misleading. He prefers to view the Spain of this epoch as a phoenix rising from the ashes, an image he borrows from Narciso Feliu de la Peña, an influential Catalan lawyer and writer. According to Kamen the decades of the 1680s, an era to which he has previously referred as that of Spain's last crisis, also marked the beginnings of a prolonged period of recovery—in the economy, population, intellectual and political life—that would come to fruition during the second half of the eighteenth century. Admittedly, signs of this recovery were not everywhere apparent and were certainly more pronounced in the Basque country and Catalonia than in the hinterlands of Castile, but Kamen's revisionist approach is long overdue. Certainly it is far more credible than traditional accounts that would have us believe that the feeble and impotent Charles II was the personification of the kingdom he ruled.

The major difficulty with the study is that the author is hard pressed to fit all of his material into his chosen theme. What, for example, is the relevance of a map (p. 287) that traces the Mediterranean travels of eight Moorish slaves captured in Valencia, 1669–86? Then too some of his material, such as the discussion of criminality in Madrid and Valencia, suggests that conditions in Spain were getting not better but worse. The lack of a conclusion is also disappointing, but on the whole this volume makes an important and original contribution to our understanding of the Spain of Charles II. No one seriously interested in the history of this period can afford to ignore it.

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JOSEFINA CRUZ VILLALÓN. *Propiedad y uso de la tierra en la Baja Andalucía: Carmona, siglos XVIII–XX*. (Serie Estudios.) Madrid: Servicio de Publicaciones Agrarias. 1980. Pp. 360. 600 ptas.

The role of nineteenth-century disentail in fostering a pattern of latifundism in southern Spain deserves detailed analysis, which this study provides for one important municipality. Carmona, thirty-three kilometers northeast of Seville on the main highway from Cordova, contains about ninety thousand hectares of the rich land typical of the area. Under royal jurisdiction during the Old Regime, it has remained rural and stable in population for centuries. These

characteristics make Carmona ideal for studying changes in land use and ownership.

Josefina Cruz Villalón has wisely chosen to analyze these changes over a long period, with chapters based on government surveys from 1755, 1850, 1910, 1946–48, and the 1970s. After a technical discussion of soil, terrain, and demography in Carmona, an exhaustive analysis of the 1755 *catastro* of Ensenada outlines the structures of land use and ownership before disentanglement. The dry-farming of grain accounted for 70 percent of the municipality's area in 1755, and another 15 percent was devoted to olives. Viticulture, horticulture, pasture, and waste accounted for the remaining 15 percent. Latifundism was already marked, with a mean parcel size of 16.9 hectares, seven times larger than the median. Large parcels were devoted more to grain and pasture than to other uses; medium and small parcels were devoted more to olives, grapes, and other labor-intensive crops. With minor changes, this pattern of land use persisted from 1755 to about 1950.

In 1755 a few large landowners (2.7 percent), mostly religious or noble, held 61 percent of the land, much of it entailed. Since the landowners tended to be absentee, they rented out their lands. Medium and small landowners were more likely to be local residents, as were the large lessees, who can be seen as agrarian entrepreneurs. Changes in landownership were dramatic with the broad disentanglement decrees of 1837, which followed earlier partial disentanglements. Ecclesiastical ownership in the 1850 survey was down to one-quarter of its 1755 level, communal property had disappeared, and the señorial regime had been dissolved. Even so, nobles remained prominent as landowners, but they were joined by a rising landowning bourgeoisie, many of whose families had previously been large lessees.

Except for a several-decade rise in olive culture, few changes occurred in either land use or ownership in the early twentieth century. Since about 1950, however, land use has experienced great and rapid change, with the introduction of new crops, modern fertilizers and machines, and the irrigation of 11 percent of Carmona's land. This has led to the first real expansion of cultivation since the Middle Ages, from 85 percent to nearly all of the available surface. Latifundism has continued to characterize the area, as it did in the eighteenth century, but the ownership patterns were fundamentally altered by disentanglement, which freed vast quantities of land for the open market and provided opportunities for the rise of a bourgeois landowning elite. Since this process was hardly unique to Spain, one could wish for more comparative material on the evolution of the agrarian economy elsewhere in Europe. In addition, recent work suggests that a nonnoble landowning elite was forming in Spain several cen-

turies before disentanglement, and an awareness of this would have strengthened the author's argument. Nonetheless, the book is a welcome addition to the literature, both for its amply documented conclusions and for its clarity of presentation and accessibility to the nonspecialist and specialist alike.

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SOLANGE DEYON and ALAIN LOTTIN. *Les "Casseurs" de l'été 1566: L'iconoclasme dans le Nord de la France.* (Le Temps et les Hommes.) Paris: Hachette. 1981. Pp. 255.

Solange Deyon and Alain Lottin, historians at the University of Lille, have written an excellent study of the "altar-breakers" of the Spanish Netherlands. Using contemporary sources, the authors reconstruct the causes of the violence against the Catholic church, the course of the destruction, and the suppression of the Protestant sects by the duke of Alba. The resulting monograph, although confined to the events of a single year in the towns along the border between France and Belgium, is a model of careful scholarship and the thoughtful application of the *mentalité* perspective to a historical problem.

According to Deyon and Lottin, the majority of the Calvinists who engaged in religious violence in 1566 worked in the nascent textile industry. The authors argue that the men and women gravitated toward Calvinism when the Catholic clergy failed to instruct them or minister to their spiritual needs. Ignored but not illiterate, the textile workers read the Bible, listened to Calvinist preachers, and developed an intense, personal religion mixed with an antipathy to the contemporary social order.

The actual attack on the altars, statues, and murals of the Catholic church represented the culmination of years of physical and mental stress among the Protestants. Rapid inflation, low wages, and a shortage of grain created an economic crisis for many urban workers. Previous decades of religious persecution, the memory of recent Calvinist martyrs, and the threat by Philip II to install a new Inquisition heightened the Protestants' fears. In addition, many workers resented the power of the absent Spanish king and the wealth and remoteness of the clergy he appointed.

In the authors' opinion, the Protestants wandered through the countryside following a spontaneous and disorganized itinerary. While traveling preachers may well have stirred summer crowds into a religious frenzy, no one dogma or order commanded the destruction of religious symbols. Instead, Deyon and Lottin conclude, the workers chose physical destruction as a "meurtre collectif" (p. 191).

They sought to purify the church, revenge their martyrs, and defeat an oppressive political power.

Indecision by the regent—Margaret of Austria—and sympathy among the nobility for the Protestants contributed to the near success of the altar-breakers in the southern provinces. Only the duke of Alba, acting for Philip II, seemed to consider the wild, diverse rampages as a national revolution. His brutal punishment of thousands of participants ensured, contrary to his expectations, that the people of the Netherlands would work to free themselves from Spanish rule and the traditional Catholic church.

Les "Casseurs" de l'été 1566 exemplifies the best use of contemporary social science as a tool for exploring history. The authors' knowledge of economic cycles and group psychology has enabled them to revise the traditional accounts and explanations of the Protestant violence. At the same time, Deyon and Lottin impose a current political perspective on their topic. Thus they see the duke of Alba as engaged in systematic repression, while the Calvinists counter "par le terrorisme" (p. 99) only to be defeated by Spanish efforts at "normalisation" (p. 109). Whether terms of such recent coinage should be applied to events so far distant is the only major question that can be raised about this fine study.

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JOHAN AALBERS. *De Republiek en de vrede van Europa: De buitenlandse politiek van de Republiek der Verenigde Nederlanden na de vrede van Utrecht (1713), voornamelijk gedurende de jaren 1720–1733*. Part 1, *Achtergronden en algemene aspecten* [The Republic and the Peace of Europe: The Foreign Policy of the United Provinces of the Netherlands after the Peace of Utrecht (1713), Chiefly during the Years 1720–33. Pt. 1, Backgrounds and General Aspects]. (Historische Studies, number 39.) Groningen: Wolters-Noordhoff or Bouma's Boekhuis, Groningen. Pp. xi, 441. f 65.

The book before us, a Utrecht dissertation, seeks to provide the background of the foreign policy of the northern Netherlands from the Peace of Utrecht in 1713 to the outbreak in 1733 of the War of the Polish Succession. Johan Aalbers announces a second volume that will recount the actual conduct of the Dutch Republic's diplomatic negotiations during these twenty years.

The Treaty of Utrecht ended four decades of almost continuous warfare against France. The exhaustion of the state finances, due to the staggering burden of the war debt, prescribed a lowered profile for the republic in the European arena. Utrecht is traditionally regarded as a major turning point. Historians contrast the dynamism and the unflin-

ing inventiveness of Dutch policy prior to 1713 with the slide into passivity and the paralysis of will that are alleged to have marked the subsequent conduct of public affairs. The evidence that Aalbers has assembled in this volume, drawn largely from the unpublished papers of the foremost political men, shows that the leaders of the republic, far from having turned their backs to the world, strove indefatigably within the limits of straitened finances to maintain a European equilibrium of power as the most effective guarantee for peace.

The greater part of this book is devoted to aspects of domestic politics that exerted a significant impact on the conduct of foreign affairs. Thus we learn about the major division of opinion within the Dutch political class between the partisans of a passive neutrality in the foreign field and the advocates of defensive alliances as the safest path to peace and security. One lengthy chapter relates the conflicts that arose within the States of Holland about the apportionment and the reform of taxation—conflicts that tended to subvert the traditional role that Holland was wont to play as the driving force within the union. Other chapters illustrate themes such as the complexity of decision making in a highly decentralized parliamentary commonwealth, the difficulties that this political system experienced in observing secrecy in the management of its foreign relations, the problem of finding suitable candidates for woefully underpaid diplomatic assignments, and the rivalry that developed among the seven provinces in the scramble for ambassadorial posts.

For all the diligence that Aalbers has displayed in attempting to elucidate the above matters, this is not a successful book. Its fundamental flaw is the lack of a valid methodology. The author largely ignores the secondary literature on his subject, so that his topics stand isolated from the historical context as well as from the continuity of time. Moreover, this neglect of the literature deprives his study of an overarching structure or central motif, leaving the various chapters with no closer connection to one another than excursions at the end of a specialized work. Aalbers does not explain why he chose to concentrate on the period 1713–33, and his selection of the latter year as his *terminus ante quem* is rather puzzling, to say the least. He has failed also to breathe any life into the foremost personalities of that era—men such as the council pensionaries Anthony Heinsius and Simon van Slingelandt and the greffier of the States General François Fagel, who seem strangely disembodied in the pages of this work. Style is not Aalbers's strong point either, for his diction almost nowhere rises above the jejune, the pedestrian, and the commonplace.

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ALBERT KERSTEN. *Buitenlandse zaken in ballingschap: Groei en verandering van een ministerie, 1940–1945* [The Dutch Foreign Office in Exile: Growth and Change of a Ministry, 1940–45]. Summary by E. BERKHOF-HAIG. Alphen aan den Rijn: A. W. Sijthoff. 1981. Pp. 468.

Institutional history and the development of foreign policy formulation and implementation are combined in this comprehensive work on the wartime Dutch Foreign Ministry in London. Albert Kersten has deftly examined and analyzed the structural and organizational aspects of the government-in-exile responding to the changing needs of diplomatic missions and consulates general abroad. It is his major contention that the Dutch designed a more centralized (both horizontally and vertically) foreign relations directorate but that numerous problems such as career recruitment, Allied communications, and unique colonial relations were not adequately resolved by this organizational division of labor.

The most fruitful and interesting portions of this careful exposition and thoroughly documented study are found in Dutch reactions to the problems brought about by the war itself. The relatively isolated and even aloof means of diplomatic relations had to be altered to make way for better coordination of activities with other Allied diplomats and representatives. The significance of devising a modern policy-making process and revamping the administrative structure is illustrated through the efforts of Van Kleffens, the foreign minister. In his interactions with the queen and the other ministers, Van Kleffens not only determined the nature and scope of the transformations but also defined the goals and means of achieving them in a rapidly changing environment.

Alongside the volumes of L. de Jong's *Het Koninkrijk der Nederlanden in de Tweede Wereldoorlog*, this work depicts and interprets the background of the institutional response of one nation to the crucial events of the Second World War. Kersten illustrates in authoritative detail the diverse challenges that small states' diplomacy had to face. His accomplishment is impressive and valuable, not only to students of modern Dutch history but also to those probing the bureaucratic side of contemporary diplomacy. This welcome book should be translated, for it will obviously reach very few written in Dutch.

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HANS CHR. JOHANSEN. *Dansk økonomisk politik i årene efter 1784*. Volume 2, *Krigsfinansieringsproblemer, 1789–93* [Danish Economic Policy in the Years after 1784. Vol. 2, Military Finance Problems, 1789–93]. (Jysk Selskab for Historie, number 37.) Aarhus: Universitetsforlaget. 1980. Pp. 161. 91.50 KR.

The introduction and the chapter headings of this second volume of Hans Chr. Johansen's trilogy on late eighteenth-century Danish financial history promise more than the book provides. The author has laid out an admirable format designed to touch all the appropriate bases, and in his text and tables he does just that—disappointingly, however, without scoring. It turns out to be a thin book with a narrow focus. The financial deliberations of a few responsible statesmen and the budgetary arrangements they arrived at are given adequate attention. The broader economic picture as well as the political counterpart is only touched upon. Although the subtitle of the volume emphasizes the problem of financing the war with Sweden (1788–89), that subject hardly receives enough attention, direct or even indirect, to merit the mention. Finally, although the period covered (1789–93) coincides with the great events, financial and otherwise, of the French Revolution, there is no effort to compare or contrast the situations and solutions in France with those in Denmark.

The book does have its merits. Specialists may be interested in some of the esoterica provided about budget categories, internal revenues, tax policies, foreign loans, interest rates, financial institutions, and the views of some of the persons making or commenting on decisions. More general readers can, provided they read Danish, obtain laconic confirmation for what they may have already guessed—namely, that the Danish financial situation improved markedly as good crops, the French Revolution, and the wars that followed altered market conditions to the advantage of neutrals such as Denmark-Norway. Johansen insists that the financial reforms adopted by the state to meet the demands of the war with Sweden also contributed importantly to stability and solvency. In the passing discussion we are, understandably, given a few insights into matters such as poor laws, road building, manufactures, and so forth. Less understandably—perhaps because the author has researched the subject for a separate publication—there is a thoroughly interesting but disproportionately lengthy vignette setting forth the economics of slavery and the slave trade in the Danish West Indies. It is a pity that the rest of the subject matter could not be fleshed out in comparable fashion.

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MATTHIAS MEYN. *Die Reichsstadt Frankfurt vor dem Bürgeraufstand von 1612 bis 1614: Struktur und Krise*. (Studien zur Frankfurter Geschichte, number 15.) Frankfurt a/M.: Waldemar Kramer. 1980. Pp. 256.

The burgher uprising against the Frankfurt city council from 1612 to 1614 (usually called the Fett-

milch uprising after its most prominent leader) was one of the most dramatic urban conflicts in early modern Germany. Here Matthias Meyn offers a general interpretation of the city's history from the mid-sixteenth century through the suppression of that rebellion. His goal is to explain the uprising with the aid of recent theoretical concepts. Specifically, he views the imperial city of Frankfurt as a "structured system" of four interacting elements—constitutional and administrative arrangements, the economy, social relations, and cultural-religious life. Meyn applies the notion of "crisis" to such a system when historical developments place its functional balance under great stress but do not challenge fundamentally, as a revolution would, the larger social order of which the system is a part. In this case that larger order turns out to be the Holy Roman Empire. Although the author does not address the issue of a general European crisis in the seventeenth century, he places local events squarely in a comparative German framework, because external pressures acted as readily as internal dysfunction to bring crisis to Frankfurt's urban system.

Meyn gives his readers an overview that draws the economic, social, political, and religious changes of the period into a meaningful pattern. He develops his analytical categories with a real sophistication free from reductionism and mere jargon. The clean, direct style, good command of secondary literature, and strong comparative perspectives make this an impressive synthesis. Strong as he is in his theoretical formulations, however, Meyn is disappointingly weak in the tasks of empirical research. Given the losses suffered in the Frankfurt archives during World War II, we must perhaps overlook the fact that Meyn produces little new evidence or information about the Fettmilch uprising. He has tried to fill gaps in the local archives by using materials in Vienna and Würzburg, but unfortunately he has still brought us little further than previous historians like Friedrich Bothe had already done before the war. More troublesome, though, is Meyn's failure to treat the substantive, empirical demonstration of his generalizations with the same rigor he demands in defining and using analytical constructs. At no point in this story of economic expansion and depression, economic competition and religious strife, social and political conflicts, outright rebellion and external intervention—nowhere does Meyn make unreasonable conclusions. But often he is short on evidence to really demonstrate those positions, or he simply leaves his analysis of substantive issues on an elusively general plane. Meyn's treatment of the elites competing for power, for example, is superficial, without the necessary social differentiations and without even a minor attempt at prosopography, although he points to their struggle as a principal aspect of systemic

imbalance in Frankfurt society. In short, Meyn's intelligence and sophistication are applied less to empirical or substantive problems than to the more abstract task of conceptualization, an area where his contributions can be of real value to future historical analysis and discourse.

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WINFRIED SEELIG. *Von Nassau zum Deutschen Reich: Die ideologische und politische Entwicklung von Karl Braun, 1822–1871*. (Veröffentlichungen der Historischen Kommission für Nassau, number 28.) Wiesbaden: Selbstverlag der Historischen Kommission für Nassau. 1980. Pp. 213.

Karl Braun is not a household name even in Germany, but in his own day he was a public figure of no little renown. Belonging to the generation that was just coming to maturity in 1848, the ambitious young lawyer and journalist had seized the opportunity provided by the revolution to launch a parliamentary career—first in his native Nassau and later in the Reichstag—that was virtually uninterrupted until 1887. During the 1850s he proved his mettle in rough-and-tumble opposition to the duke of Nassau's reactionary and pro-Austrian policies, and by the end of the decade he was also playing a significant role on the national stage. Above all, in 1859 he became president of the newly founded Congress of German Economists, a position that he was to hold for the next twenty years. This important organization provided a platform from which leading spokesmen of the liberal bourgeoisie set forth their *laissez faire* ideas. Braun was also active in the influential *Nationalverein*, which, during the 1860s, agitated for a parliamentary Germany under Prussian leadership. But once Bismarck began to shape the Reich according to his own ideas, which included the incorporation of Nassau into Prussia, Braun enthusiastically supported him. Throughout these years Braun was pouring out a steady stream of books, pamphlets, and newspaper articles. The list of them fills five pages of Winfried Seelig's bibliography.

Braun's ideas were neither profound nor original, but they have had durability, as anyone who reads his pronouncements side by side with those of Ronald Reagan will readily perceive. He was confident that the cure for poverty was to be sought through the release of creative economic energies in a free society, that in a truly free society there would be harmony of class interests, and so on. In later years he edited the writings of John Prince-Smith and Frédéric Bastiat, like-minded devotees of the creed of the so-called Manchester party.

Obviously Braun is a worthy subject for scholarly

investigation, but Seelig regrettably has not made much of his opportunity. True, he has been handicapped by an almost total dearth of private letters by Braun; consequently, in sketching "the ideological and political development" of the man, he has had to rely almost exclusively on Braun's writings for a public audience. A more serious handicap, however, is the author's lack of imagination in going about his job. Numerous subjects cry for an attention and analysis that they do not receive—for example, there is no adequate treatment of the relation of Braun's activities in Nassau to the specific local constituencies, economic and otherwise, that sustained them. In this respect the study by Nicholas Martin Hope, *The Alternative to German Unification: The Anti-Prussian Party in Frankfurt, Nassau, and the Two Hesses, 1859–1867* (1973), is much superior. The most successful part of the book is that dealing with the years immediately preceding the founding of the Reich; its chief value, however, consists in the digests of Braun's principal writings down to 1871.

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HANNES SIEGRIST. *Vom Familienbetrieb zum Managerunternehmen: Angestellte und industrielle Organisation am Beispiel der Georg Fischer AG in Schaffhausen, 1797–1930*. (Kritische Studien zur Geschichtswissenschaft, number 44.) Göttingen: Vandenhoeck und Ruprecht. 1981. Pp. 293. DM 78.

Hannes Siegrist's study of the Swiss iron and steel firm Georg Fischer opens with an intriguing question: How did Switzerland, with its paucity of natural resources and limited domestic market, manage to industrialize in the course of the nineteenth century? His supposition is that management may have been crucial; thus his succinctly written case study concentrates on entrepreneurial leadership and decision making.

Siegrist begins with the metal workshop of Georg Fischer in 1797 and moves briskly (one hundred years in twenty-five pages) to the major enterprise employing more than eight hundred workers in several branches in 1896. Critical for the firm's success in the early years were the Continental System, with its temporary exclusion of British exports; the decision to abandon brass, copper, and bronze for iron and steel; and Fischer's decision to employ his sons in important positions rather than move toward a partnership with strangers. For this reason Fischer gave his sons a technical education in Vienna.

Although the business grew by leaps and bounds, especially in the boom years between 1850 and 1873, Fischer's son and grandson maintained a very personal style of management into the 1880s, including a patriarchal concern for widows and work-

ers incapacitated by illness. In the prosperous years after 1894 a sizable white-collar force developed, divided between a sales and an administrative staff. Numerous workers were promoted to foremen, and graduate engineers were employed as Georg Fischer III sought to increase efficiency to meet British and German competition. The 1890s constituted a crucial decade of transition, because the growth of the firm to more than six hundred workers meant that few now saw their employer frequently; the yearly Christmas party was the last lingering evidence of the patriarchal past. Although refusing to deal with a union, management did meet regularly with a workers' committee confined to company employees. Unable to finance further expansion from family capital, Georg Fischer III transformed the business into a corporation with shareholders in 1896. The late nineties saw decision making shift to the board of directors, whose members represented Switzerland's major banks. Differences of opinion between the board and Fischer led to his removal from any effective managerial role by 1902. Modern methods of bookkeeping as well as American ideas of shop management were introduced after being adapted to Swiss conditions.

Siegrist sees significant social mobility from the shop floor to white-collar positions but little movement within the varied layers of the white-collar structure. It should be stressed that a highly skilled worker earned more than half the white-collar employees did before the war. After the war some concessions were made to the office and sales staff to avoid any radicalization. Workers and white-collar employees never came together to make demands on the firm. The latter crossed picket lines readily during strikes and remained passive despite numerous grievances.

Although the book is cogently written and clearly organized, it leaves one somewhat dissatisfied. The question posed at the outset is never directly addressed. We do learn that the firm had high international standards of management and that it was continually being rationalized to enhance efficiency. One would expect the conclusion to analyze Swiss techniques of management, as contrasted with those of Germany, Great Britain, or the U.S.; one finds, however, only a summary of the book's main points. Nevertheless, as a detailed case study, the work is well conceived and executed and should be very useful to scholars interested in entrepreneurial history.

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W. L. GUTTSMAN. *The German Social Democratic Party, 1875–1933: From Ghetto to Government*. Boston: George Allen and Unwin. 1981. Pp. xii, 362. \$37.50.

W. L. Guttsman's work will likely disappoint those readers who seek a clear, chronological account of the rise and fall of the German Social Democratic party (SPD). Each of the chapters covers a theme such as political mobilization of the working class, party organization, socialist subculture, and Social Democratic theory and tactics. Sometimes the boundary lines between chapters are not clearly demarcated. Even within each chapter Guttsman does not strictly respect chronology. Still, with some effort one can discern an interpretation of the SPD's historical evolution.

Industrialization and urbanization, combined with social antagonisms and political persecution, created a working-class ghetto within the Second Reich. Guttsman reviews the array of techniques with which the SPD exploited that situation and built itself into a formidable political machine. Blending a broad range of recent scholarship with material from published primary sources (there is little evidence of archival research), he pursues the interpretation of Günther Roth and Dieter Groh that the Social Democratic movement provided negative integration for the German working class. The host of organizations and activities gave many workers an outlet for their energies and their aspirations for collective and individual improvement. Although the party was formally committed to a full socialist transformation of society, it did nothing to bring about revolution. Guttsman does not, however, go so far as to argue that the SPD hindered the growth of revolutionary sentiment during the Second Reich; indeed, he believes that until the immediate postwar years the cautious approach of party leaders corresponded by and large to the sentiments of the working class (pp. 262, 331–32). Similarly, he argues that the growth of an extensive bureaucracy did not destroy the internal mechanisms for democratic control, for the party remained dependent on, and responsive to, its functionaries. The party organization is described as a functionaries' democracy (p. 244).

Despite the emphasis on preserving and expanding the organization, which was shared by the more than thirty thousand volunteer workers, political disputes were not uncommon within party ranks. The cleavage was generally not between party leaders and the grass roots but between conservative and leftist forces, each with their own representatives at various levels of the organization and their own geographical base of support. Guttsman discounts the significance of embourgeoisement of party leaders.

Guttsman's discussion of socialist activities in the realm of culture and education represent a useful addition to the existing literature in English. His analysis of the SPD's parliamentary efforts is thin. Specialists may also note a few slips, such as the

implication that Philip Scheidemann and Gustav Noske *voluntarily* retired from party offices to enjoy government administrative positions (p. 259).

Guttsman uses statistics, but not correlation and regression techniques, to analyze voting results, membership, and occupation. In his view, the SPD expanded its working-class base only marginally over time. Although further efforts to broaden the party were appropriate, he finds no simple solutions for the party's difficulties.

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OLLI KAIKKONEN. *Deutschland und die Expansionspolitik der USA in den 90er Jahren des 19. Jahrhunderts: Mit besonderer Berücksichtigung der Einstellung Deutschlands zur spanisch-amerikanischen Krise.* (Studia Historica Jyväskyläensia, number 20). Jyväskylä, Finland: Jyväskylän Yliopisto; distributed by Jyväskylä University Library, Jyväskylä. 1980. Pp. viii, 209.

This Finnish doctoral dissertation, published in German, is an examination of German–United States relations during the period of the Spanish–American War. Olli Kaikkonen employs mainly German diplomatic and newspaper sources for his primary evidence, relying on the major secondary interpretations for his general discussion of American expansion.

In the central part of the study, Kaikkonen describes the evolution of German governmental policy toward the United States in the late 1890s and then of German press opinion of American foreign policy. The main German concerns about United States policy (aside from American economic protectionism, which was the prime determinant of German–American relations) revolved originally around American intentions toward Cuba and Samoa. The emergence of American expansionist aims in the Philippines took the Germans by surprise in 1898. According to Kaikkonen, American expansion was never a really major concern of either the German government or the German press in the 1890s. Once it became clear in 1897 that British policy would prevent joint European action to protect Spain's colonial interests, the German government adhered strictly to a position of neutrality in the Spanish–American conflict in the hope that it would permit Germany to pick up a few new colonial possessions in compensation for American gains. The majority of German newspapers, though unfriendly to America, fully supported this position. Foreign Secretary von Bülow was, nevertheless, unable to secure part of the Philippines as a German naval base or, in 1898 at least, to use the compensation argument to annex Samoa. His purchase of the Carolines and most of the Marianas from Spain was

a questionable gain. Kaikkonen also argues that negative German newspaper attitudes toward American policy led to an anti-German reaction in the American press and eventually to minor German governmental efforts to improve Germany's image in the United States.

Kaikkonen provides an able summary of events and a useful bibliography. Except for his overview of German editorial positions, however, it is not clear that he adds much that is new to our knowledge of either German or American imperialism or the Spanish-American War. His attempt to "quantify" press reactions is half-hearted and superfluous. His approach is largely narrative; although he suggests that the developments he describes may have some relevance for understanding *Weltpolitik* and the relationship between German imperialism and domestic politics, he makes no real effort to show what the relevance is. Although Kaikkonen summarizes some of the major positions in the vigorous debate on the nature of Wilhelmine imperialism, he gives only the barest hint of how his work might affect that debate. He does state that he is dealing mainly with immediate, as opposed to long-term, aspects of imperialism, but too often this appears to be an excuse for summarizing editorials and diplomatic documents without adequate analysis. In future work on this subject, one hopes that the author will attempt to grapple with the broader issues that his first book suggests but does not pursue.

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San Antonio

PETER D. STACHURA. *The German Youth Movement, 1900–1945: An Interpretative and Documentary History*. New York: St. Martin's Press. 1981. Pp. x, 246. \$22.50.

In his introduction to this study, Peter D. Stachura announces two goals: to provide the first general investigation of the German youth movement and to offer an "interpretative synthesis" (p. 7). If he is more successful in achieving the first goal than the second, the fault lies in part in the subject itself. In his brief conclusion, Stachura fails to fit the pieces together. German youth movements would have been a more suitable title.

In the book's first section, Stachura surveys the development of the prewar German youth movement, the rise of the *Bündische Jugend* in the Weimar period, and confessional and political youth organizations. This section is based primarily on published sources, and scholars familiar with the literature in the field will not find much that is new. Nevertheless, this section is a very useful reference on the pre-1933 youth groups and their impact on education, culture, and German life.

The section on the *Bündische Jugend* effectively illustrates the discontinuities in the history of youth organizations. The group loyalty stressed by the *Bündische Jugend* contrasts with the individualism of the prewar youth movement and the mass regimentation of the Hitler Youth. Stachura differs from other scholars in his denial of any role of the *Bündische* groups as precursors of National Socialism, in spite of their *völkisch* ideology. He faults the *Bündische Jugend*, who had their own elitist vision of the future, only for their failure to recognize the dangers of National Socialism.

In the second part of the book, based largely on archival research, Stachura breaks new ground with his discussion of the Hitler Youth after 1933. The author stresses the role of the Hitler Youth as an independent and activist force within National Socialist society. He chronicles the Nazi youth movement's successful efforts to monopolize youth activities after 1933 and to establish itself as an educational institution parallel to and competitive with the school system. Stachura shows how the Hitler Youth used activities and ideas fostered by pre-1933 youth movements to achieve its own quite different ends. In doing so, he effectively illustrates the lack of continuity between the pre-National Socialist groups and the Hitler Youth. In spite of its largely youthful leadership and its ideology of youth, the coercive and hierarchical character of the National Socialist youth movement diverged sharply from its predecessors' voluntarism. The Hitler Youth effectively destroyed the youth movement. It came close to destroying the educational system as well.

Stachura writes fluently and clearly. It is a pity that his documentary appendix falls short of this standard. There is little excuse for translations like "We are through our blood nationalist" or "The Deutsche Freischar is a group of people that wants . . . to lead its life towards itself and the nation in unconditional personal freedom together with a strong sense of responsibility . . ." (p. 171). A clear rendering in English would have better served Stachura's effort in the appendix to convey the diverse nature of the youth groups.

MICHAEL STEPHEN STEINBERG
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RODERICK STACKELBERG. *Idealism Debased: From Völkisch Ideology to National Socialism*. Kent, Ohio: Kent State University Press. 1981. Pp. xiii, 202. \$18.00.

Roderick Stackelberg sets himself the task of exploring "the insidious complex of traits that made *völkisch* ideology so seductive to generations of middle-class Germans" (p. ix). He approaches his topic biographically: in succession he studies the lives, thoughts, and works of Heinrich von Stein, Fried-

rich Lienhard, and Houston Stewart Chamberlain. He credits Stein, the Wagnerian aesthete, with originating the "völkisch perversion of idealism." Like his friend and idol, Stein taught that regeneration from modern forms of materialism could be achieved in art and religious experience. Lienhard was more political and less sensitive than Stein; Stackelberg characterizes him with Fritz Stern's word, a *Vulgär-idealist*. Finally, Houston Stewart Chamberlain built the bridge from the conservatism of Wilhelmine society to National Socialism. The author's conclusions—sketched in the introduction—seem correct. These writers lived amid the collapse of an old order: "they sought to salvage a culture that would buttress authoritarian politics in a traditional societal order." Their works allow us a view of "the evolution of idealism into a defense against the disruptive consequence of modernization and industrialization in the late nineteenth century" (p. 13).

Stackelberg frankly admits his debt to Fritz Stern's *Politics of Cultural Despair*, especially in the use of biography to illuminate the history of social thought. But he parts with Stern over the place of conservative criticism in Wilhelmine society. Stern saw his cultural pessimists as outsiders in imperial Germany; in many ways they were. Stackelberg's figures were "respectable" men: Chamberlain, for example, was highly honored both by the Germany of the kaiser and that of National Socialism. Stackelberg puts himself on the side of those who discover the intellectual origins of National Socialism within German cultural-intellectual traditions—however "debased" those traditions became. "[Nazi] fascism reaffirmed in radical and populist guise the values of aristocratic conservatism that *völkisch* authors transmitted from an earlier age" (p. 157).

The study's model of interpretation—the revolt against modernity—has now found an honorable place in German scholarship. Stackelberg applies it rather uncritically, especially in view of the recent studies of labor historians, which argue that same hypothesis to explain working-class militancy. The sections on Stein and Lienhard are informative, but we do not learn much new about Chamberlain from this study. The general organization is logical and clear. Perhaps the "lesson" of his study, as he characterizes it, makes his work especially profitable reading today. "By denying or concealing the role of self-interest in human motivation, idealism (like religious fanaticism) can breed hypocrisy and lend itself to cynical manipulation for political purposes" (p. xii).

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VOLKER WÜNDERICH. *Arbeiterbewegung und Selbstverwaltung: KPD und Kommunalpolitik in der Weimarer*

Republik; Mit dem Beispiel Solingen. Wuppertal: Peter Hammer. 1980. Pp. 290.

"Why Solingen's local history?" Volker Wunderlich poses this question at the outset of his new study (p. 9), but in the course of the work he leaves the answer unclear. The participation of the Solingen branch of the German Communist party (KPD) in local politics is only the first, if the most extensively treated, of three major topics. There follow sections on the programmatic turns the KPD took in regard to *Kommunalpolitik* between 1919 and 1933 and on the significance of the institutions of municipal self-government in the Weimar Republic's struggle for survival. A brief "outlook on theory" substitutes for a conclusion.

All these topics are of interest, but can they be connected successfully? For Wunderlich the link between them is what he terms a "local process of fascization" (pp. 14, 100), a phenomenon of bourgeois desertion of the republic commencing in the late 1920s. Solingen provides an example of a community where the Communists should have been strong enough to resist this development. Their electoral following ranged from two to four times that of the local Social Democrats; in 1930 their branch leader was chosen mayor—a unique occurrence in Weimar Germany. Yet, Wunderlich argues, they never had a policy that could make their strength effective. His analysis of the weakness of KPD programmatic statements is designed to illustrate why. His probe of municipal self-government is intended to show what was at stake—a set of representative, if "bourgeois," institutions that provided a crucial line of defense for working-class interests during the final Weimar years.

But why should German Communists have cared about bourgeois institutions, especially given the Comintern's leftward turn in 1928? Wunderlich's answer amounts to a judgment that if they had anticipated Nicos Poulantzas's *Faschismus und Diktatur* (1973) they would not have "repeatedly underestimated the significance of political and ideological factors in historical development" (p. 173). That is the difficulty with this book. If one does not share Wunderlich's view that Poulantzas has "demonstrated" the character of the *Faschisierungsprozess*, that events "verify" his analysis (p. 232), then the coherence of the work breaks down. The section on Solingen becomes an episode in working-class politics, neither more nor less revealing than others. The unit on Communist policy seems an ahistorical search in an area the KPD always considered secondary. Regarding local self-government Wunderlich offers some trenchant, if not original, observations: the Social Democrats, particularly state officials in Prussia, frequently ignored democratic procedures; the real power of communal represen-

tatives had been eroded by the decrees of the national and state governments even before the Nazi *Machtergreifung*. But were these events steps in a *Faschisierungsprozess* rather than continuations of conflicts that went back to the empire? The question is especially pressing in Solingen, where the Nazis themselves gained support relatively slowly. Wünderlich is correct in affirming that political struggles can be analyzed most concretely on the local plane (pp. 9, 266). His own interpretive framework, however, will be convincing only to exponents of a Marxist theory that explains, with the *Besserwisserei* of academic hindsight, why people did not do what proper *praxis* demanded.

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ERNST WILLI HANSEN. *Reichswehr und Industrie: Rüstungswirtschaftliche Zusammenarbeit und wirtschaftliche Mobilisierungsvorbereitungen, 1923–1932*. (Wehrwissenschaftliche Forschungen, Militärgeschichtliche Studien, number 24.) Boppard am Rhein: Harald Boldt Verlag. Pp. x, 260. DM 38.

The role of German industry in the clandestine efforts by that country's military leaders to circumvent the disarmament provisions of the Versailles Treaty during the Weimar Republic has long been the subject of accusations and speculation. Drawing mainly on the records of the *Heereswaffenamt*, now located in the military archives at Freiburg, Ernst Willi Hansen has provided the first extensive, documented study of this controversial subject.

Hansen takes as his point of departure the concept of the "military-industrial complex" developed in the U.S. after the Second World War, but his findings demonstrate the inappropriateness of that concept for the Weimar case. As he repeatedly shows, the military alone provided the driving, directing force behind attempts to update Germany's industrial war potential and prepare for an eventual expansion of production far beyond the quantitative and qualitative restrictions of the peace treaty. Far from seizing the initiative to spur economic preparations for war, as Marxist writers have repeatedly alleged, industry proved sympathetic but less than eager and, in some cases, outright balky. Large, prominent manufacturing firms in particular frequently showed a reluctance to become involved in undercover violations of treaty restrictions, at least until the late 1920s, when the cabinet—though not the parliament—assumed a share of responsibility for the military's projects. A number of considerations played a part in this, including a mistrust of the military bureaucracy as a result of experiences with *dirigisme* from that quarter during World War I and a concern for the maintenance of trade secrets.

But Hansen attributes the hesitant or negative attitude of a number of major firms toward clandestine armament production or planning mainly to their concern about the effects of disclosure on their export markets, especially in the countries that had fought against Germany in the war. Compared with the magnitude of possible sales losses abroad, the small contracts and modest subsidies the military could offer frequently proved an insufficient enticement. In attempting to analyze the varying responses of industrialists, Hansen invokes a shopworn dichotomy, contending that iron and steel firms readily cooperated with the military, whereas those in the fields of chemical and electrical production held back. But his own findings, which make Krupp appear almost as balky as I. G. Farben but reveal greater willingness to cooperate on the part of the Siemens electrical equipment firm, cast doubt on any such simple classification formula.

Although Hansen does not emphasize the point, his evidence reveals that the military relied primarily on small and medium-sized firms, not the giants of German industry, for its rearmament projects, especially those of a covert nature. Considerations of security governed this choice in part, since the odds for maintaining secrecy seemed better in smaller plants in provincial towns than in large industrial complexes in the urban centers of labor militancy. But geographical considerations weighed heavily, too, in view of the vulnerability of the large-scale enterprises of the Ruhr and the Rhineland in the event of an invasion by the Western powers.

Overall, Hansen concludes that the results of the military's efforts to stimulate industrial preparations for rearmament remained disappointingly meager, despite claims to the contrary in Georges Castellan's *Le réarmement clandestin du Reich* (1954), which was based on French intelligence estimates. The basic difficulty lay in the unwillingness of the civil authorities to meet the Defense Ministry's requests for funds for that purpose, especially after the world economic crisis struck Germany. On that count, Hansen ventures the interesting speculation that Brüning's drastic curtailments of the Reichswehr's proposed budget may have influenced General Schleicher's decision to bring the chancellor down in May 1932.

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ILSE MAURER and UDO WENGST, editors. *Politik und Wirtschaft in der Krise, 1930–1932: Quellen zur Ära Brüning*. In two volumes. Introduction by GERHARD SCHULZ. Assisted by JÜRGEN HEIDEKING. (Quellen zur Geschichte des Parlamentarismus und der Politischen Parteien, Die Weimarer Republik, number 4.) Düsseldorf: Droste. 1980. Pp. cxxxvii, 854; xxii, 857–1593. DM 396 the set.

A government in the rigid pursuit of a balanced budget, cutting everything except the military titles; an industry deeply split but demanding political action at any cost to improve industry's profitability; a trade union movement immobilized and scared by the rising number of unemployed; representatives who were split not only between left and right but also between "responsible," but unattractive political bargaining and "irresponsible," but popular ideological politics; a banking community that stayed aloof of more mundane economic and political struggles, relying on the unassailable powers of the central bank and its international network until it came crashing down; and an agricultural set whose blind egotism was hardly justified by a truly desperate economic plight—this is the net result of 533 documents on no fewer than 1,539 pages that deal with one of the more historiographically embattled periods of the German past, the Brüning chancellorship between 1930 and 1932. Ilse Maurer and Udo Wengst's editing of these documents from some fifty-odd different sources was a monumental task. One can only hope that these two volumes are more often used than others in the same series.

Some of the findings, one might expect, will be easily assimilated. They mainly have to do with the rise and demise of Brüning and are outlined in a cautious and careful introduction by Gerhard Schulz. Thus it should be beyond doubt that the collapse of the Weimar coalition in 1930 was not caused by the luckless Social Democratic ministers who resisted a raise in the social insurance premium. Their lack of political fantasy and their honest worries about the issue were elements in stratagems to establish a "presidential regime"—not just a presidential government, but an antiparlamentarian and antilabor rule—even though it was not at all clear what it should and could do. Brüning, who emerged as the new chancellor more by default than by design, however, does not appear to have been the programmatic politician who steadfastly guided German policy toward the ultimate goals of a restoration of German economic and military might. Unquestionably he had designs, but incrementalist politics rather than programmatic policy prevailed in his dealings with the largely unforeseen economic and political consequences of the worsening crisis. Even the much-taunted policy on reparations—as Schulz and many documents point out—was a means to evade the contradictions of domestic affairs and to find the smallest common denominator for any politics. Brüning was not a second Iron Chancellor.

Once we cast his own image aside, Brüning, in fact, gains in stature: very ambitious, quite ruthless, and an excellent tactician in the daily political struggle, he was able to hold together a governmental system in full decay for two years against expecta-

tions that gave him a mere few months. When he was forced out, the potentials of presidential regimes were exhausted as well. The need to rebuild the economy and the political process almost from scratch was so urgent that political and economic antagonisms—kept in balance by Brüning's tactical ingenuity—could no longer be papered over. When the struggle for a future German state and economy began in all seriousness and bitterness in the fall of 1931, Brüning's days were numbered.

The larger issues at stake go beyond these and similar debates. It becomes evident that we have only a very limited understanding of the political process during the period despite an unusual richness of documents. What exactly were the features of the struggle for power? Obviously we can personalize or instrumentalize politics, as Schulz does in his introduction, and thus gain some narrative coherence. Hugenberg, for example, with his radical "ideological" stance, was at odds with almost any other political grouping, whereas Westarp pursued a "moderate" policy of interest representation; hence the DNVP fell apart, and with it one more pipe dream of bourgeois unification. But this is not more than the description of a significant pattern that is repeated on all sides. Its meaning in the process of acquiring power during the crisis we do not understand. We are inclined toward standard interpretations of a time in which the very process of the formation of alliances and the very definition of what constitutes politics—that is, some of the basics of any political process—were questioned and embattled.

Moreover, we are only beginning to understand the relations between economics and politics in this period, despite valiant efforts to analyze the linkages between various industries and industrialists on the one hand and individual politicians and political groups on the other. The majority of documents address this issue. We should recall, though, that the main debate at the time was whether industry should or could rely on the state, and if so, what kind of state it should be and what guarantees it had to provide to make this relation work. Once again, one could turn to a straightforward analysis of interest politics, as Schulz suggests. But for industry at large the question was no longer one of interests alone, but of the state's role in capital accumulation. It is not at all evident who was on which side in this struggle.

These questions have been approached before. Asking them once more just means to invite the reader to approach this magnificent collection with an open mind. The selection lays some old problems to rest—mainly those concerning Brüning—but creates a whole series of new ones dealing with the German state and the German economy in crisis. I should warn in this respect of at least two limitations

of the selection: the omission of foreign (economic) policy as well as the economies of industry. The volumes provide very little on these two aspects. But that should not detract from their extraordinary value—not only for those who work in the field but also for anyone who wants to learn more about crisis in an advanced industrial state.

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PIERRE AYÇOBERRY. *The Nazi Question: An Essay on the Interpretations of National Socialism, 1922–1975*. Translated by ROBERT HURLEY. New York: Pantheon. 1981. Pp. xiii, 257. Cloth \$15.95, paper \$6.95.

This is a formidable effort to explain Nazism through a history of the works that have been written about it. Beginning with the Nazis' own perceptions of themselves, the twelve chapters of this book trace the major interpretations of Nazism throughout the past six decades. Pierre Ayçoberry proceeds in a roughly chronological fashion, with 1945 marking the logical break between the two major parts, entitled "Analyses for Action" and "Settling with the Past," respectively. The 1960s represent another period of transition, since the waning of the Cold War gave rise to more sophisticated interpretations than the totalitarian models of the 1950s and their rigid Soviet counterparts. The works of more than one hundred authors are surveyed at a lively, sometimes dizzying pace. Of course, there are omissions (George L. Mosse's works on *völkisch* ideology and Dietrich Orlow's party history come to mind), but on the whole Ayçoberry's selections and classifications are judicious, and his summaries are critical but fair.

Although it will be useful primarily to scholars, *The Nazi Question* is written clearly and engagingly enough to be of interest to nonspecialists as well. Among the prewar authors who receive the most extensive treatment are Hermann Rauschning, Edmond Vermeil, Daniel Guérin, Wilhelm Reich, Ernst Bloch, and Franz Neumann, whose *Behemoth* Ayçoberry terms the "first of the classics" (p. 97). Postwar authors who merit the most detailed attention are Hannah Arendt, Carl J. Friedrich, Karl D. Bracher, Ernst Nolte, Tim Mason, Reinhard Kühnl, Nicos Poulantzas, and Ralf Dahrendorf. If Marxist theories seem to be overrepresented, especially for the earlier period, it is because these provided (and continue to provide) the major stimulus for scholarly inquiry and debate. From the start the crude dogmas of the Third International offered a target for attack. The doctrine of "social fascism" of the 1920s and the "agent theory" of the 1930s were effectively refuted by more rigorous Marxist analyses. Since *The Nazi Question* deals exclusively with

Nazism, the first full-length historiographical study to do so, Ayçoberry does not need to address the current debate on fascism as a generic concept. There are few other controversies that he avoids, however. The debate on the complex relationship between Nazism and capitalism is well aired, as is the equally controversial question of the extent and homogeneity of the Nazis' middle-class constituency. The dispute generated by A. J. P. Taylor's *Origins of the Second World War* is well presented, too.

In any study of Nazism, the problem of continuity is a primary issue. In view of the variety of his material and his self-assumed role as impartial arbiter between competing theories, it is not surprising that Ayçoberry ends on a noncommittal, even skeptical, note: "One cannot say for certain whether the Third Reich was a radical departure from, or a continuation of the preceding regimes" (p. 225). Perhaps this is because it was a good deal of both. Indeed, if there is a major theme that emerges in this book, it is the recurring dualisms of Nazism: its conservatism and its radicalism, elitism and populism, modernization and antimodernity, dynamism and defense of the status quo, capitalism and statism, order and disorder, fanaticism and opportunism. Nor does Ayçoberry hold out much hope that quantitative studies will resolve these contradictions. Ultimately it is ideological assumptions that determine where the explanatory emphasis falls. Interpretations from the left will continue to stress the collusion of traditional elites, whereas interpretations from the right will accentuate the revolutionary temper of the Nazi movement. The reader looking for definitive answers is likely to be disappointed by this book, but the scholar in search of questions to pursue will find many provocative leads.

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JAMES M. RHODES. *The Hitler Movement: A Modern Millenarian Revolution*. (Hoover Institution Publication, number 213.) Stanford: Hoover Institution Press. 1980. Pp. 253. \$14.95.

James M. Rhodes synthesizes positions previously presented by Norman Cohn and Eric Voegelin. A clear-cut thesis is presented: National Socialism was a millenarian movement with Adolf Hitler as its prophet and a theology structurally similar to medieval Christian heresies and the Johannine tradition. Specifically, Rhodes enumerates six themes found in all three movements: the view of life as a catastrophe, the experience of sudden revelations that explain sufferings and promise release, the discovery of the existence of a demonic conspiracy, the belief in their own sacred mission to battle the conspiracy, the preparation for an imminent Arma-

geddon, and the expectation that paradise on earth will follow their victory. The Nazis were able to exploit the German grievances and alienation in the post-World War I period and put them into a neo-Manichean race cosmology. The ideology of heroic fanaticism and the *Volk* were in German thinking for more than a century, but National Socialism placed it in a millenarian context, and that made it a mass movement.

A chapter on dialectical selfhood is neither as clearly presented nor as carefully interwoven with the book's central theme. The Nazis, in their personal lives, had experienced rejection, either by parents or society. This left the Nazis with a burning desire to become white knights, sacrificial heroes, or at least members of an elite group.

The conclusion is that such a movement could recur in the future. National Socialism and the other examples of millenarianism demonstrate a pattern of how human societies respond to the disaster syndrome if they are cursed with the sins of sloth and pride.

The book should be of interest not only to students of the Third Reich but also to sociologists, political scientists, historians of religion, theologians, and psychologists. Each of these groups, however, will find something to criticize. Historians can chastise Rhodes's very selective use of historical events, Nazi propaganda, and diaries. The brief work is not a comprehensive history but an attempt to give the best evidence to support a thesis. Likewise, many of the aspects of other millenarian movements that are not analogous to National Socialism are not discussed. Theologians may object to some of Rhodes's doctrinal positions, which sometimes heavily affect his analysis. Political scientists and sociologists will find that Rhodes avoids some of the theories and quantitative analysis now in vogue. Psychologists will be less upset with Rhodes's de-emphasis of Freudian interpretations (following a most evenhanded evaluation) than with his nineteenth-century comprehension of paranoia and "mass insanity." But the clarity of Rhodes's thesis makes the book worthwhile. It can stimulate some good interdisciplinary reflection on National Socialism if the reader can avoid a picayune approach.

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FRIEDHELM GOLÜCKE. *Schweinfurt und der strategische Luftkrieg 1943: Der Angriff der US Air Force vom 14. Oktober 1943 gegen die Schweinfurter Kugellagerindustrie*. (Sammlung Schöningh zur Geschichte und Gegenwart.) Paderborn: Ferdinand Schöningh. 1980. Pp. 444. DM 38.

Shortly after the German defeat in 1945, Albert Speer, the "Wunderkind" of the Munitions and

Armament Ministry in the last three years of World War II, told his Royal Air Force interrogators that a systematic bombing of the German bearing industry would have brought the entire German armament production to a standstill within four months and an end to the war in six months. The October 14, 1943, attack by the U.S. Army Air Force on the bearing industry in Schweinfurt was the culmination of the American doctrine of strategic airpower—the knockout blow that would have fatally crippled the German war machine. The raid on Schweinfurt, however, became the "Black Thursday" of the USAAF, and the staggering losses led to a discontinuation of further air strikes against the bearing industry and a crisis in Allied air strategy that was not resolved until 1944, when aircraft improvements and a new operations plan were ready. As Speer later wrote, not one tank, aircraft, or other product fewer was manufactured because of the shortage of bearings, and the Germans were able to recover from the 60 percent loss of manufacturing capacity as a result of the respite afforded by the American regrouping.

Friedhelm Golücke's *Schweinfurt und der strategische Luftkrieg 1943* is the first in-depth scholarly analysis of the air war against the bearing industry from the perspective of both the Allied attackers and the German defenders. The author has attempted with success to use the Schweinfurt raid as a case study for the history of the air battle over Germany. Golücke reconstructs the actions of both sides on the strategic and tactical levels and provides a unique description of the bombing of October 14 as seen from the opposing participants in the air and the defenders and civil population of Schweinfurt. Golücke succinctly analyzes the development of the American air forces and problems of daylight bombing operations as well as the intentions and decisions made by the leaders of the Luftwaffe faced with the difficult task of Reich air defense. He concludes that the bearing industry was probably the point where air power could have been decisive, but only if the other Allied armed forces were in a position to take advantage of the German weakness. The most successful application of strategic air power in World War II was the creation of favorable combat conditions for the ground forces through direct attacks on the German military—perhaps the most important factor in the Allied final victory.

Although less so with American sources, Golücke has made extensive use of the available primary German materials. The sad state of the limited Luftwaffe archives and the scattered location of captured documents continue to present a difficult task for the historian of the German air force during the years 1914–45. The value of this book lies in its whole less than its parts, and scholars will find it useful for the suggestion of further topics (for example, the issue of National Socialist leader-

ship and air defense) as well as for the detailed representation of an important and hitherto less-explored chapter of the Second World War. Readers will also find useful technical descriptions and charts on the bearing industry and the bombing damage from the October attack. Golücke's work reflects one of the major trends in German military history today—the investigation of the close ties between industry and the military and the impact of modern warfare on the economy. New research such as Horst Boog's *Die Deutsche Luftwaffenführung* (1979) and the projected ten volumes of the Militärgeschichtliches Forschungsamt's *Das deutsche Reich und der Zweite Weltkrieg* (1979) expands on many of the themes of this book.

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BRADLEY F. SMITH. *The Road to Nuremberg*. New York: Basic Books. 1981. Pp. 303. \$14.95.

Bradley F. Smith, author of books on the childhoods of Hitler and Himmler and a history of the Nuremberg Tribunal's proceedings, now describes the process by which the U.S. government arrived at its policy for the postwar trials of the Nazi leaders. Smith is at his best when he details the day-by-day search in Secretary Stimson's War Department for a viable alternative to summarily shooting Nazi leaders. The result was the "ingenious" but "fairly radical" legal concept of a charge of conspiracy in criminal organization. This would try party and state agencies before an international court, and if the court found an organization guilty, then in subsequent trials the prosecution would only have to prove participation in the condemned group in order to have individuals judged criminal. A second legal innovation of great future importance was the charge of conspiring to wage aggressive war. Curiously, this indictment floated in and out of the planning with little stress being placed upon it.

Smith shows the slow, awkward development of the original proposal of Lieutenant Colonel Murray C. Bernays, War Department legal specialist, as the policy was moved through channels by Assistant Secretary of War John McCloy. The plan was changed and modified because of events such as the Malmédy massacre, the discovery of concentration camp atrocities, the sudden end of the European war, challenges from the Judge Advocates Department, minor objections from the Navy and State Departments, and major opposition from the Treasury Department under the leadership of Henry Morgenthau, Jr. Morgenthau continually proposed—and at the Quebec Conference achieved—endorsement of a harsh and quick judgment on the Germans. Executions, not trials, were Morgenthau's wish.

Smith judges that America's postwar trial planning was at times disorganized, shortsighted, and naive but did produce a more rational alternative than general executions. Summary hangings, as Smith points out, would certainly have resulted in the unjust killing of innocent individuals, would not have allowed consideration of degrees of guilt, and would not have provided a record of Nazi evil.

This book will fascinate the lawyer by its skillful exposition of legal intricacies. It will delight political historians by its detailed description of the Byzantine labyrinths of the Washington bureaucracy during the World War II era. The author's image of Roosevelt's procrastination, Hull and Stettinius's lack of leadership ability, Stimson's strength, and Morgenthau's righteous wrath fit in well with the portraits painted by others. The general reader, however, is not clearly informed how this planning did or did not influence the actual trial. Historians might be concerned about what a critic of a previous book characterized as Smith's "cavalier treatment of secondary material." An almost absolute avoidance of noting these sources tends to give the impression that everything the author writes is new, whereas a good deal of the material and the interpretations have been employed before. These are minor difficulties in a book that must be praised for a skillful synthesis presented with clarity and elegance. The road to Nuremberg was indeed a circuitous one, with many detours, switchbacks, and cul-de-sacs. Smith is a sure guide on the entire route.

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KENDALL L. BAKER *et al.* *Germany Transformed: Political Culture and the New Politics*. Cambridge: Harvard University Press. 1981. Pp. xvii, 381. \$25.00.

Quantitative historians may be interested in knowing that at long last the first book-length description of the development of postwar West German political culture has appeared. Entitled *Germany Transformed*, it analyzes public opinion data on the civic attitudes of West Germans, using time series, principal components analysis, betas, and multiple classification analysis, among other methods. The authors, Kendall L. Baker *et al.*, are three political scientists who, unlike historians, make no effort to tell the story of the political transformation of the Federal Republic but instead test a number of hypotheses of their choice regarding whether "a viable democratic political culture has developed" (p. 69) there in place of what earlier commentaries have dubbed "an economy in search of nationhood." It is a work of considerable merit, although traditional historians may groan at the heavy use of jargon and the overt rattling of the gears and clanking of the chains of the research operation, especially since this is not exactly a work from which the curious novice could

learn much about the procedures of such research. Moreover, the formulations used to posit lines of inquiry frequently make one wish for fuller explanations—for example, when German industrialization and “the appearance of an influential middle class” are dated to have occurred long after “the rest of Europe” (pp. 3–4), and “modernization” is said to have begun in 1945 (p. 6).

The book’s strength lies in its plotting of longitudinal trends in German political culture. The rising frequency of political discussion, the increasing sense of political efficacy among the citizenry, the declining influence of social class on voting, and the changing nature of partisanship are admirably detailed. Not all the findings are beyond controversy, nor are they all equally well documented. The complexities of political realities sometimes resist the researchers’ hypotheses. The heavy jargon is always relieved by good summaries at the end of each chapter and by a concluding section that puts the findings into the context of future developments.

An endeavor of this sort could hardly help having some flaws, including the choice of questions asked or omitted. Its most serious shortcomings stem from the limited nature of the data base, which only rarely includes the 1950s and, therefore, for the most part shows trends covering only the period from 1961 to 1972. Thus, in spite of an epilogue on the 1976 elections, there is an appearance of rather short-term developments—leading up to the 1972 SPD landslide—being interpreted as the end-all of all future political change to come (pp. 255, 285). We are made to believe, without convincing proof, that many of the new patterns—activist youth, the new issues of lifestyle, and an accommodationist foreign policy—are here to stay. There are also some flaws in historical perspective, pertaining in particular to the “early Federal Republic generation” and its concerns and antecedents. The book’s analysis of generational economic and personal deprivation, for example, underestimates the trauma suffered by the populations expelled from Eastern Europe or directly affected by the war (in all, about one-fourth of the total) as compared with the generations of the 1920s and 1930s, experiences that were later reflected in the conservatism and security-mindedness of the Adenauer years. The authors also hold a rather stereotypical view of the social policies of those years and seem unaware of the profound antitotalitarian effect of the older generations of the time (pp. 12–13, 54–55). There is little reflection of the extraordinary voter appeal of Adenauer’s policies reintegrating a defeated and discredited Germany into the family of nations or of the fact that the *Ostpolitik* of recent years was possible only because of the security policies of the 1950s.

Such misunderstandings also leave their mark on the distinction between “old politics” and “new

politics” issues that the authors unfortunately made central to their work and methodology. “Old politics” issues here include not only the “materialist” bread-and-butter interests of the Ronald Inglehart thesis but also a “need to maintain a traditional social order and domestic tranquillity,” the “preservation [?] of national sovereignty,” and the goal of reunification (p. 141). “New politics” issues likewise go beyond the “postmaterialist life-style” to include *any* concern with education (pp. 244–48) and the desire to improve relations with *any* foreign countries, even the U.S., thus robbing the distinction between “old” and “new” of its discriminatory power.

More specific methodological problems involve the generational analysis that should control the age factor. After all, could not the otherwise “materialist” Weimar generation have been just as “postmaterialist” in its salad days as the student youth of today? (pp. 151, 221). Similarly, one can hardly hope to ascertain whether a person spent his or her adolescence in economic security by that person’s present occupation, considering the extraordinary changes of the Nazi, war, and postwar years, especially among the refugees (p. 152). Furthermore, a reader may ponder whether the presumed effect of political socialization in an economically secure environment is not contradicted by the fact that so many of the young rebels feel a compulsion to see the world in terms of crisis and “borrowed misery,” to borrow Helmut Schelsky’s words. Perhaps this is the result of guilt feelings induced in the young by parents raised in hardship themselves.

All in all, this book raises more questions than it can answer. In another decade, perhaps, the authors will undertake a new assault on the same subject, but this time with the help of data covering a longer period.

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FRANZ QUARTHAL. *Landstände und landständisches Steuerwesen in Schwäbisch-Österreich*. (Schriften zur Südwestdeutschen Landeskunde, number 16.) Stuttgart: Müller und Gräff. 1980. Pp. xxxviii, 514.

The last two decades have witnessed the appearance of numerous monographs on the corporate bodies of the principalities of the old Holy Roman Empire. This study by Franz Quarthal deals with the estates and diet of the Habsburg possessions in Swabia from the late Middle Ages to their absorption by Württemberg in 1806. Throughout its history Austrian Swabia was one of the Habsburgs’ smallest and most remote territories. Consisting of dozens of scattered enclaves, it constituted only the central portion of the so-called Austrian *Vorlande*, which also included the Vorarlberg and the Habsburgs’

Rhenish possessions. By the sixteenth century, however, it had emerged as an autonomous province, largely because its diet was the territory's only existing administrative body. Hence, the Landtag served not only as the crucible for crown-country relations but also as the sole avenue for the dispatch of Habsburg domestic policy.

Like most early modern representative bodies, the Landtag owed its emergence to its sovereigns' need for extraordinary revenue. After examining the Habsburgs' piecemeal acquisition of their Swabian territories, the author traces the development of the estates and also the development of the diet as a tax-granting institution. Given the Habsburgs' growing military needs against their Turkish, Protestant, and French adversaries, it is not surprising that taxation became progressively more heavy and less extraordinary. Moreover, under Maria Theresa the crown finally began to exercise oversight in tax collection and administration and even reduced the estates' autonomy within the *Vorlande*.

Yet what Quarthal finds most significant is that the Habsburgs and their Swabian estates weathered these developments with a minimum of conflict. He attributes their remarkable cooperation to a number of factors. He points out, for example, that the estates limited their ambitions to negotiating, voting, and administering taxes and never challenged the Habsburgs' religious, foreign, or domestic policies. Meanwhile, the crown demonstrated its appreciation for the estates' cooperation and indispensable administrative services by moderating its fiscal demands and respecting both their autonomy and their right to vote all tax levies. In addition, the two sides continually worked together in trying to enlarge membership in the diet: the crown in order to expand the tax base; the estates in order to reduce tax rates by spreading the existing tax obligation among a larger area. In fact, with Maria Theresa's successful incorporation of numerous districts that had previously been *reichsunmittelbar*, per capita taxation actually fell despite considerable increases in the overall tax yield.

The book is based on a solid bibliography of published sources and unpublished material drawn from sixteen family, municipal, and regional archives. The author would have served the reader better, however, had he also explored the extensive new literature on the corporate bodies in the other German states and Habsburg dominions. Such a comparative approach would have certainly afforded this study a broader perspective and relevance for students of both early modern German and Austrian institutional history. Indeed, the author himself might have been surprised to learn that Austrian Swabia was not the only German or Habsburg territory in which there was a solid tradition of cooperation between representative bodies and the

crown. He also might have devoted fewer of the book's five hundred pages to the minutiae of each village's tax obligations, especially since little attempt was made to make meaningful use of the variations between the different localities. These shortcomings notwithstanding, this book represents a welcome addition to the growing corpus of institutional studies of the old Reich.

CHARLES INGRAO
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BRUCE F. PAULEY. *Hitler and the Forgotten Nazis: A History of Austrian National Socialism*. Chapel Hill: University of North Carolina Press. 1981. Pp. xxi, 292. \$19.00.

Bruce F. Pauley has already contributed to the understanding of Austrian National Socialism with his study of the Styrian Heimatschutz. In the present work, he sets himself a more difficult task: not only has he traced the development of the Austrian Nazi party from its foundation in the era of Georg von Schönerer in the late nineteenth century to the German occupation of the Austrian Republic in 1938, but also he has investigated the factors that helped the party to maximize its political effort. The volume narrates remarkably well the trials and vicissitudes of the beleaguered party, which during the twenties played an impotent political role. The absence of an effective leadership, the intraparty feuding, and factionalism seriously hampered the growth of Austrian National Socialism from the very beginning. From 1930 onward, the author discerns a change. The impressive success of the Nazi party in Germany and the appointment of Theo Habicht as the new party leader in 1931 helped consolidate the movement and attract popular support. After the Nazi success in the 1932 Austrian elections and in the wake of the takeover in the Reich, there was no realistic alternative for the Austrian promoters of German unity other than to accept Nazi leadership. The bloody failure of the July 1934 putsch caused the moderate Nazi elements to recognize that Anschluss could be achieved through evolution not force. Ultimately, Hitler aligned himself with a group of more sophisticated young Carinthian party officials, who after 1934 embarked on the untried path of promoting Nazi goals outside of the illegal party. The new flexible policy line of gradual subversion of the Schuschnigg regime set in motion a development that gave Hitler the opportunity to seize power in 1938.

The author has followed a largely chronological approach but has also probed with insight and learning some topics separately, such as the party's propaganda and its social composition. The result is an informative, well-written book. Its analytical clarity was made possible by the thorough consultation

of archival materials. The study is, however, not entirely convincing in its concluding part. It is difficult to accept Pauley's formulations of the conflict between the leadership of the party in the Reich and the illegal Austrian party (p. 193) or of the indifference of the Austrian Nazi leaders to Keppler's views on March 11, 1938 (p. 211). The old militants around Josef Leopold, who traditionally resorted to direct actions, were not able to develop the understanding of the instrumentalities of the new gradualist semi-legal policy. As soon as Hitler was able to decide on a deliberate policy, he took complete control of the chain of command and abandoned his aversion to intervening in favor of one of the many rival factions. Leopold's political fate was sealed in 1936 and 1937 after the new strategy and tactics elaborated by bright SS planners and their Austrian aides had been integrated into the July 1936 agreement between Germany and Austria. When in July 1937 the Führer entrusted SS-Gruppenführer Wilhelm Keppler with the handling of Austrian affairs, the majority of the illegal party was relegated to a secondary role. The SS chose Friedrich Rainer, Odilo Globocnik, and Arthur Seyss-Inquart to implement the evolutionary course.

This carefully printed volume has a brief note on the unusually rich sources and a good bibliography. The index is adequate and the illustrations helpful. As the first full account of Austrian National Socialism, the study contributes to a better knowledge of the complex Nazi phenomenon.

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ERIC COCHRANE. *Historians and Historiography in the Italian Renaissance*. Chicago: University of Chicago Press. 1981. Pp. xx, 649. \$37.50.

Eric Cochrane has produced a comprehensive, clearly organized, and much needed survey of Italian Renaissance historians. Few will challenge his claim to have accounted for 90 percent of the historians and histories written in the Italian Renaissance. Cochrane is certainly justified in hoping that his book will be used both as a substitute for Fueter's classic work and as a guide to Muratori's collection of Italian historians.

Cochrane's scholarship is truly prodigious. He has read not only the Renaissance histories but also the enormous body of secondary literature on the subject. The footnotes are replete with references to archival documents, works still in manuscript, and even book reviews of secondary material. The historians themselves are treated with loving detail. Despite the formidable problems of organization presented by the subject, the book is clear and straightforward, although the author's method

tends to fragment the basic historiographical themes and often leads him to devote equal space to minor and major historians.

Cochrane's stress on comprehensiveness includes a positivist bias. He is not unsympathetic to rhetorical history. In fact, he claims the study taught him "that history can be both aesthetically pleasing and politically *engagée* without ceasing to be scientifically accurate" (p. xix). But his standards of historical accuracy seem limited to modern canons of scholarship. He often castigates his subjects for deficiencies in source criticism, poor research methods, and even ignorance of economics. His doubts about the lasting value of his material are so pronounced that at one point he finds himself wondering why anyone has bothered to translate Poliziano's history of the Pazzi conspiracy for modern readers of English.

His interest in factual material leads Cochrane to ignore some of the most important interpretations in the secondary material. Although he uses William Bouwsma's impressive study of Venice as a source for minor details, the interpretation of Sarpi that constitutes the climax of that work is passed over. Bouwsma presents an exciting and well-documented picture of Sarpi's work as one of the masterpieces of European historiography, whose sophisticated historical vision expresses Sarpi's debt to the political culture of the Renaissance. By contrast, Cochrane sees the work as an example of the split between ecclesiastical and political history, a split Sarpi breached only by such superficial means as substituting disputations for battles and sermons for orations. Cochrane's picture is too narrow and unconvincing, given the well-documented, broadly significant vision found in Bouwsma's work. In a similar vein, the section on Guicciardini would be stronger if Mark Phillips's work had been treated more sympathetically.

Historians and Historiography of the Italian Renaissance certainly casts light on many aspects of its subject. The sections on Giovio and on universal history are interesting, and the section on the antiquarians presents a remarkably positive picture of these writers. Nevertheless, the book does not present a new general interpretation of Italian Renaissance historiography. Readers conversant with recent literature in the field will be familiar already with the list of achievements Cochrane identifies in the epilogue. Despite these interpretive shortcomings, the work has lasting value. It provides in a single volume useful introductions to an immense variety of writers, genres, and periods.

DONALD J. WILCOX
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MARIA LUISA CAVALCANTI. *Le relazioni commerciali tra il regno di Napoli e la Russia, 1777-1815: Fatti e teorie*. (Biblioteca dei "Cahiers Internationaux d'Histoire

Économique et Sociale," number 29.) Geneva: Librairie Droz. 1979. Pp. 394.

The Academy of Sciences of the USSR and the Italian National Council for Research subsidized this volume of the *Cahiers Internationaux d'Histoire Économique et Sociale*. Although the main pattern of Italo-Russian relations has already been sketched by Giuseppe Berti (*Russia e stati italiani nel Risorgimento* [1957]), Maria Luisa Cavalcanti here covers the period 1777–1815 in far greater detail. The volume is based on archival sources in Naples, Moscow, Leningrad, and Paris. In addition to abundant contemporary narratives, Cavalcanti has also utilized the major secondary sources for the period.

In the eighteenth century, both Russia and Naples expanded trade. Russia was especially interested in developing its Black Sea commerce and sent the first ship from St. Petersburg to Naples in 1764. Official relations began in 1776. The first Neapolitan plenipotentiary sent to St. Petersburg was Muzio de Gaeta, duke of San Nicola; his counterpart was Count Andrej Kirillovich Razumovskij. Both envoys reached their posts in 1779 but were soon replaced—by Antonio Maresca Donnorso, duke of Serracapriola, and Pavel Martinovich Skavronskij, respectively. When the American Revolution broke out, Catherine II induced Naples to join the League of Armed Neutrality. In 1783, Naples sent a cargo to St. Petersburg of macaroni, citrus fruit, nuts, jam, soap, essence of bergamot, and silk goods. Russia reciprocated with timber. The two countries signed a commercial treaty in 1787. Most interesting was a secret clause whereby Russia pledged to furnish its own mercantile flags to Neapolitan ships so that they would enjoy reduced tolls on wine at the ports of Kerson, Sebastopol, and Teodosia. The French Revolution and the Napoleonic wars prevented the renewal of this twelve-year treaty.

The last good year of Russo-Neapolitan commercial relations was 1805. In February 1806 Naples was incorporated into the Napoleonic empire and was ruled by Joseph Bonaparte, followed by Joachim Murat. The period from 1807 to 1815 was very difficult for Russo-Neapolitan trade because of the French factor in both countries. Ambassadors were exchanged but Napoleon thought the Neapolitan mission to Russia was useless. Consequently, in 1812 Naples followed France in breaking relations with Russia. Murat allied with Austria in 1814, rejoining Napoleon in May 1815 only to be captured and executed before the year was over. After the war Ferdinand IV was restored to his throne. In need of grain, Russia again sought Neapolitan trade, which was generally resumed in 1816.

Photographs and many tables of commercial data enlighten this narrative. The appendix carries a twelve-page table showing the comparative states of

navigation and commerce of various countries at the port of Naples from September 1 to October 1, 1803, and a fifteen-page table indicating Russian ships and cargo frequenting the port of Messina from 1802 through the first trimester of 1806. Over two pages explain the money, weights, and measures of Russia and of Naples. The index is of names only, but the concluding table of contents lists the main topics covered in each of the eight lengthy chapters.

Many a decade will pass before this book becomes obsolete. It will hold a place of honor with Berti's book, mentioned above, and with such other classics on Italo-Russian relations as Ernesto Artom's *L'azione della Russia a favore dell'indipendenza italiana* (1903), covering 1815–60 with accent on 1854–59; Carlo Morandi's *Le relazioni tra l'Italia e la Russia dal 1900 al 1917* (1949); and the large, multi-authored volume edited by the History Institute of the Academy of Sciences of the USSR, *Rossia i Italia* (1968).

MARY PHILIP TRAUTH
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PAOLA NOTARIO. *La vendita dei beni nazionali in Piemonte nel periodo napoleonico, 1800–1814*. (Studi e Ricerche di Storia Economica Italiana nell'Età del Risorgimento.) Milan: Banca Commerciale Italiana. 1980. Pp. 649.

With this work, another excellent volume has been added to the distinguished series of Banca Commerciale studies on the economic history of Italy during the Risorgimento. As its title suggests, this book treats a narrow topic in a vast field, but it is an exemplary specimen of masterfully executed micro-history. Although the findings are quite finite and directed to the specialist, Paola Notario has given us an easily comprehended work that—probably unintentionally—invites scholars from broader and related fields to read it and speculate on the new meanings to be found in some of the sweeping socioeconomic transformations attempted in the past.

Notario opens her narrative with a useful, concise background sketch of the condition (in all senses) of real property in the eighteenth century, pointing out that "revolutionary" confiscations of real estate began decades before the French Revolution. Appropriation of that land by monarchies, particularly church lands, was undertaken to remedy the chronic deficits of the royal treasury. When continued and expanded by revolutionary governments, particularly that of Napoleon in Piedmont, the practice was hardly new and still served primarily to pay government expenses and old debts, not to redistribute land to any particular social class for ideological reasons.

With exhaustive and meticulous research carried

on in a number of untapped archives, Notario scrutinizes the nature of Piedmontese land ownership and the characteristics of various properties, as well as government policies and activities from the arrival of French administrators in Piedmont in the Jacobin period until Napoleon's downfall. The nine laws issued between 1799 and 1810 that regulated the sale of lands are analyzed in detail and the results of their application minutely evaluated. Within that framework, Notario develops and comments upon the progress of the program, the contracts that regulated the sales, the purchasers and their manner of payment, and the possible reasons why some laws functioned well and others wholly failed to meet their purpose. Broadly speaking, she finds the most activity and greatest successes (from the government's viewpoint) between 1800 and 1804. Reaching the end of her period, however, she concludes that the overall program was at best a partial success. Some arrangements for sales failed completely, and the campaign was terminated by other events.

More significant than a summary of the volume's content is the two-fold contribution that Notario has made to historical knowledge. First, this is monumental and masterful research and reporting for economic historians, particularly those specializing in the modern period. In addition, more than 350 pages of documentation covering each and every contract is provided, as well as information on as many of the purchasers who have left a trace by which they can be described. In short, the topic is covered quite definitively.

Second, but of broader importance and appeal, is the social history to be derived from Notario's study and data. Her findings, refined by state-of-the-art computer technology, report a stunningly small number of purchasers (a mere 0.2 percent of Piedmont's inhabitants) and show the less than revolutionary nature of those who bought (nobles, professionals, and other landowners lead in that order). Although this purports to be no more than economic history, the book misses an excellent chance for much more exhaustive comment on social implications of the land transfers. But the raw material is here for others who wish to carry on the work.

Notario's style is clear and eminently readable. Competence, caution, clarity, and confidence distinguish her research, opinions, analyses, and writing.

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PIETRO STELLA. *Don Bosco nella storia economica e sociale, 1815-1870*. (Centro Studi Don Bosco, Studi Storici, number 8.) Rome: LAS. 1980. Pp. 652. L. 24,000.

As a historical form, biography is troubling. Biography begins and ends, as Benedetto Croce noted, with historical accidents, and one life may touch a myriad of historical problems without providing the material to explore any of them. Pietro Stella recognizes these difficulties and deals with them in his new book by reversing the usual approach. In 400 pages of text (supported by a 200-page appendix), he uses the archival flotsam and jetsam of Saint Giovanni Bosco's busy life to write not a biography but a wide-ranging series of analytic and perceptive comments on the significance for social history of the documents he has found. To read the book is like taking a seminar that proceeds by *explication de texte*, the text being notes and letters that reveal traces of dialect, lists containing the names of donors and students and clergy, scraps of budgets, Don Bosco's plans for publications, some pedagogical exercises, and much more. Wherever there are numbers, Stella adds and averages and compares; wherever there are names, he looks at age and longevity and social origin. Rarely is the evidence on a single point complete, but each finding and each clue is carefully related to the historical literature on the social structure, religious practice, and economic development of nineteenth-century Italy.

Such a book represents a luxury not available for many topics or to many scholars. This is the eighth volume in a series published by the Centro Studi Don Bosco, the seventh of the series to deal with Don Bosco (none as biography), and the fourth of them to be written by Stella. The achievements of Saint Giovanni Bosco, his ministry to the poor of Piedmont, and the rapid growth and spread in Italy, Europe, and America of the Salesian order he founded are all remarkable in the religious and social history of the nineteenth century.

In the 1840s Don Bosco's efforts were concentrated in the northern outskirts of Turin (there are maps and pictures), which were then thickening with workers and peasants seeking work. There he founded his first Oratory. There his young co-workers and the boys to whom they gave lodging, guidance, education, and some manual training were, like Don Bosco himself, largely the sons of peasants, more comfortable in speaking Piedmontese dialect than the Italian they were taught. Don Bosco's reputation for doing something to help the abandoned urban poor attracted money from the charitable leaders of the city and from pious aristocrats. His cause and his personality attracted young clerics, and he effectively used the social network reaching into the clergy and the aristocracy that he had established as an outstanding student in Turin's seminary and as a young priest. As education became the central mission of his Oratory, however, the sons of the lower middle class came to predominate over migrant workers. The printshop, where

religious tracts and schoolbooks were published, became by far the most important of the many artisanal activities. Soon provincial cities unable to meet the demand for increased education called on Don Bosco to take over their floundering schools.

Through this period of growth Don Bosco learned the value of picking towns with good economic prospects and a railroad, learned to buy and sell property with a careful eye and to manipulate long-term loans, and learned to rely on good connections within the church and the government. But he held property in his own name (or that of other priests), making intelligent use of the laws of liberal capitalism to avoid the constraints that would have followed from absorption into the parish structure or from seeking formal recognition from the state. Increasingly his funds came from lotteries, from the contributions of fellow clergymen, and from bequests. His ornate church of Maria Ausiliatrice was a center of popular devotions and thaumaturgical appeal for much the same reason that his publications stressed simple language and practical instruction.

Piece by piece, Stella shows all this and far more, resting each point on (often incomplete) documents carefully read, tossing in much additional interesting information about migrants (girls born in the city tended to have different names from their immigrant mothers), journalism, financial arrangements, salaries, pedagogy, and religious practice. The account stops with the 1870s because the documentary sources change then (with the establishment of the Salesian order) and because, Stella argues, Don Bosco's practices had been established by then. With great fairness and insight Stella lays out the elements of a major historical synthesis—the achievement of a method that in itself cannot lead to synthesis or to a book that will find many readers.

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FRANCESCO BALLETTA. *Le Due Sicilie e l'Egitto nel secolo XIX: Contributo alla storia delle relazioni economiche internazionali*. (Istituto Italiano per la Storia dei Movimenti Sociali e delle Strutture Sociali, Biblioteca dei "Cahiers Internationaux d'Histoire Economique et Sociale," number 27.) Geneva: Librairie Droz. 1979. Pp. 262.

This is the history of a nondevelopment, commerce between the Two Sicilies and Egypt from 1815 to 1860. As Francesco Balletta admits, during much of this period trade between the two countries was nonexistent (p. 122). Apart from imports of Egyptian commodities during the famine years 1816–17 and rare shipments of assorted Italian products to Egypt, the only commercial link was through the

leasing of Neapolitan vessels for Egyptian trade with other countries. How tenuous a connection this was can be judged by the appearance of only two Neapolitan ships in Alexandria from 1836 to 1840. Neapolitan shipping was small compared to other Italian shipping, let alone British, French, Austrian, and Russian shipping.

Balletta believes that the proximity of the Two Sicilies and Egypt, together with Mohammed Ali's efforts at economic and military modernization, should have stimulated trade between southern Italy and Egypt. He attributes the absence of commerce primarily to government policy in both countries. The restored Neapolitan Bourbons felt compelled to emphasize good relations with the great powers, knowing only too well that they regarded Mohammed Ali's efforts at economic modernization and military expansion as a threat to their increasingly advantageous arrangements with the Sublime Porte. Naples's reluctance also stemmed from concern over depletion of raw materials, fears of famine (which led to restraints on agricultural exports), and anxiety over cholera epidemics. Mohammed Ali's efforts to manipulate Egyptian commodity prices in order to maximize profits, although often unsuccessful, periodically inhibited trade.

Balletta, however, underestimates economic considerations. He concedes that the economies of the Mezzogiorno and Egypt were not complementary (especially overlapping in agricultural production) but gives insufficient weight to the competitive disadvantage of southern Italian industry in winning Egyptian contracts. Despite some progress, it remained inefficient and undercapitalized; it was certainly no match for British, French, and other European producers. That other European merchants and manufacturers were not deterred by political considerations from entering the Egyptian market also weakens Balletta's stress on the Bourbons' desire to curry favor with the great powers as an obstacle to trade with Egypt.

The absence of linkage between the economies of southern Italy and Egypt is reflected in this work's structure. The author is constrained to write alternating parallel summaries of social and economic developments in Egypt and the Mezzogiorno to explain policy formation in each country. Although admittedly posing awkward methodological problems and exceeding the book's intended scope, a comparison of development patterns would provide greater integration. A tedious chronicle of ships flying the Bourbon flag, arriving in and departing from Egyptian ports (with the name of each ship's captain!) provides the only thread of continuity. The many statistical tables, although informative, are also repetitive.

These deficiencies must be weighed against some good qualities. This work is well written and reflects

archival research in London, Paris, Marseilles, and Naples. The bibliography contains the appropriate secondary sources. Nevertheless, most readers will find themselves better served by broader treatment in Pasquale Villani's *Mezzogiorno tra riforma e rivoluzione* and David Landes's *Bankers and Pashas*. Balletta should devote his talents to writing about what did happen rather than what did not.

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JOSIP ADAMČEK. *Agrarni odnosi u Hrvatskoj od sredine XV do kraja XVII stoljeća* [Agrarian Relations in Croatia from the Middle of the Fifteenth to the End of the Seventeenth Centuries]. Summary in German. (Gradja za Gospodarsku Povijest Hrvatske, number 18, or Odjel za Hrvatsku Povijest, Centar za Povijest Znanosti, Monografije, number 8.) Zagreb: Sveučilišna Naklada Liber. 1980. Pp. 853.

This voluminous work by Josip Adamček is a thoroughly documented economic history of the late feudal period in an area severely affected by the Turkish conquest. Even though the author presents a wealth of detailed information drawn from primary sources, he never loses sight of the whole, synthesizing the material at the beginning of each chapter or subdivision as well as in a concluding chapter. As regards territorial limits, the area treated does not comprise the whole of modern Croatia: the exclusion of eastern areas devastated by the Turks as well as the exclusion of Dalmatia limits the coverage to so-called Old Croatia and to parts of Slavonia.

The periodization used in this work follows changes in the form of feudal rent. By the second half of the fifteenth century the money economy had been considerably expanded due to the development of peasant trade in agricultural commodities. As a consequence, a major part of the feudal rent came to be fixed and collected in money. By the 1540s the rise in the price level experienced in Western Europe reached Croatia, which introduced the second period, during which dues fixed in money were replaced by rent in kind. In a parallel move the nobility entered the trade in agricultural commodities. The stimulus was provided by increased demand from lands west of Croatia as well as from the military garrisons in the Croatian territory. Through a series of institutional changes the nobility succeeded in gaining a virtual monopoly in trade: with the aid of the Croatian diet (*sabor*) the landlords limited the extent of peasant trade by securing preemptive rights for purchases of wine, grain, and other local commodities as well as for purchases of imported goods; the collection of duties was enforced with greater rigor, but the

nobility gained exemption from such levies; the privileges of town dwellers were restricted, so that they became subject to the same feudal dues as dependent peasants; finally, the nobility obtained leases from the church for the collection of the tithe, in which the lease payment represented only a small portion of the tithe collected in kind from the peasantry. Needless to say, peasants resisted the worsening of their lot by means that ranged from petitions addressed to the crown to armed uprisings.

The seventeenth century ushered in the third phase in the devolution of the feudal rent. During this period the share of allodial properties (*demesne*) was increased and with it the share of agricultural output produced directly by the landlords. Additions to *demesne* lands came from clearings, from the reoccupation of lands freed from the Turks, or simply from appropriation of peasant holdings by the landlords. The labor needed for cultivation was secured by increasing the labor rent (*corvée*), which in turn necessitated imposing new restrictions on the mobility of the peasants.

The author views the entire process as one of retrogression. It prevented the evolution to capitalism, the preconditions for which were laid during the fifteenth century with the emergence of peasant trade and of an embryonic town economy. In this respect the Croatian experience finds parallels elsewhere in Eastern Europe (Leonid Zytkowicz, "The Peasant's Farm and the Landlord's Farm in Poland from the 16th to the Middle of the 18th Century," *Journal of European Economic History* [1972]: 135–54).

One could argue that it was the evolution toward capitalism in Western Europe that plunged Eastern Europe into retrogression. The reasoning for such an argument is that the rise of the nonagricultural production in Western Europe and a parallel expansion of trade determined the comparative advantage of Eastern Europe in agricultural production. The merit of Adamček's excellent scholarship is that it throws light on the largely negative consequences of such a relationship, at the same time as it provides the basis for integrating the developments in Croatia into the broader framework of European economic and social history.

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WOLF DIETRICH BEHSCHNITT. *Nationalismus bei Serben und Kroaten, 1830–1914: Analyse und Typologie der nationalen Ideologie*. (Südost-Institut München, Südosteuropäische Arbeiten, number 74.) Munich: R. Oldenbourg Verlag. 1980. Pp. 423. DM 98.

In this monograph, an expanded version of his doctoral dissertation, Wolf Dietrich Behschnitt traces the evolution of nationalist goals and programs among the Serbs and the Croats in the years

between 1830 and 1914. Although both nationalities had separate and distinct national movements, their cultural identity and territorial aspirations overlapped. Religion clearly divided them: Croats are Roman Catholics, and Serbs are Orthodox Christians. Language did not: their modern literary languages were based on variants of the same Southern Slav dialect (*štokavski*) and were for a long time considered one language (Serbo-Croatian or Croato-Serbian). Neither nationality lived in a compact ethnic area. Serbs formed a sizable minority in Croatia, Slavonia, and Dalmatia, and both Serbs and Croats claimed Bosnia-Herzegovina. The Serbs had a small principality, later kingdom, that, as it became independent from Turkish rule, could expand to include all Serbs, whereas the Croats had only a semiautonomous kingdom in the Habsburg empire as their political core. Many of the nationalist programs, especially those of the Croats, recognized the close ethnic ties between Croats and Serbs and included elements of Yugoslavism. Behschnitt is most interested in tracing and evaluating the balance between primary national and Yugoslav goals. Such an analysis is useful to an understanding of the unsolved national conflicts that plagued the Yugoslav state after its formation in 1918.

Behschnitt defines the types of nationalist goals as "Croatism" and "Serbism," "Greater Serbia" and "Greater Croatia," Panserbism and Pancroatism, and four types of Yugoslavism: unitary, integral, federative, and "pseudo" Yugoslavism. He then applies these categories to an analysis of the programs of key nationalist leaders and groups. For Serbia he has chosen Ilija Garašanin, Vuk Karadžić, Svetozar Miletić, Svetozar Marković, the *Narodna odbrana* and *Ujedinjenje ili smrt*. For Croatia he selected the Illyrian movement, Bishop Josip Strossmayer and Franjo Rački, Ante Starčević, the Social Democrats, and the *Nacionalistička omladina*. Where he considers it relevant, Behschnitt discusses the role of language, religion, economic development, and political and constitutional change.

Although the text moves slowly from program to program, striving to develop a clear statement of goals from writings that are often hazy and contradictory, Behschnitt's footnotes give depth and perspective. They include detailed summaries of the literature on the topic and lively discussions of traditional and recent historiographical debates.

This book is not a study of the dynamics of nationalism; it is limited to the stated goals of individuals and groups. Behschnitt's approach, therefore, is quite traditional, despite his efforts to frame the subject with modern methodology. This book should be of interest to scholars of Southern Slav and East European history and to those working in the field of comparative nationalism. Perhaps the greatest value of this book is that it makes

available a complex body of materials, both programmatic and historiographic, in a major European language.

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N. BĂRBUȚĂ and N. BOCȘAN. *Independența româniei în opinia belgiană* [Rumanian Independence in Belgian Opinion]. Foreword by JACQUES WILLEQUET. Summary in French. Cluj-Napoca: Editura Dacia. 1980. Pp. 323. 10 L.

Rumanian historians have been diligent in their search of foreign archives for material to document and memorialize the history of their country. Not only have these scholars returned with note cards in hand, but also they have obtained microcopies of vast numbers of documents in many languages, which enhance the holdings of the State Archives in Bucharest. The volume under review is one small product of this research. It contains, first, a short monograph (in Rumanian) tracing Belgian diplomatic and press response to the events surrounding the preparation, proclamation, and recognition of Rumanian independence (May 9, 1877). Admittedly this is not an important topic in itself, but it does call attention to analogies between these two small nations emerging under the shadow and influence of the great powers. N. Bărbuță and N. Bocșan demonstrate that these similarities were apparent to European contemporaries and that especially by Rumanian statesmen, Belgium was often viewed as a model for the political and constitutional organization of the Rumanian state. Of more interest is the second part of this volume, which reproduces 89 diplomatic documents and 41 press extracts for the period January 1877 to February 1880. All are in the original French with a summary in Rumanian. Almost one half of the diplomatic documents and about one third of the press extracts originated in Bucharest or Iași and often contain valuable eyewitness accounts. Of special interest are the reports of Rumania's participation in the Russo-Turkish War and the relationship between Rumania and Russia during that period. Documents originating elsewhere in Europe throw light on the attitudes of the great powers toward Rumania and its bid for independence. The monograph contains helpful bibliographies (in footnotes) listing previously published documents relating to the diplomatic and press history of the period of independence and the surprising number of previous studies of Belgian relations with Rumania in the period 1859–1878. With the publication of this volume, however, one must conclude that the topic has been exhausted.

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VENIAMIN CIOBANU. *Relațiile politice româno-polone între 1699 și 1848*. [Rumanian-Polish Political Relations, 1699–1848]. Summary in French. (Biblioteca Istorică, number 53.) Bucharest: Editura Academiei Republicii Socialiste România. 1980. Pp. 238. 11.50 L.

National concerns, rivalries, and hostilities have often prevented the scholars of Eastern Europe from fruitfully analyzing interregional relationships. One area, however, that has had a tradition of productive study is Rumano-Polish relations, both on the Rumanian side (P. P. Panaitescu, L. Boicu, and Ilie Corfus among others) and on the Polish side (M. Handelsman and St. Lukasik are representative). Veniamin Ciobanu's new book is a useful and interesting addition to that tradition.

The work is divided into three distinct parts. The first is an extended discussion of the literature on Rumano-Polish relations between 1699 and 1848 and provides an excellent précis for each relevant study. As might be expected, the bulk of the investigation on this era deals with the post-1800 period.

The second part treats the extremely interesting eighteenth century in East European history between the Peace of Karlowitz (1699) and the partitions of Poland (1790s). The author does an excellent job of relating the policies of the Polish state and the Rumanian principalities to the immediate pressures of a period of vast transitions: the disappearance of Transylvania as an independent entity, the decay of the Ottoman empire, the fading of Sweden, the rise of the Russian and Habsburg empires in Eastern Europe, the continued decline of Poland, and the establishment of Phanariot rule in the Rumanian principalities. At the same time more distant factors, such as the advent of revolutionary France, are given just attention.

One particularly noteworthy aspect of the book is the author's contribution to the recent trend toward "Phanariot revisionism" in Rumanian scholarship. He is at pains to depict the international and even the internal policies of the Greek princes in Rumania as stoutly defensive of the *de jure* (if not *de facto*) rights of the principalities in the Ottoman orbit.

The focus of the third part is on the efforts and impact of the Polish political emigration in and on Rumania in the nineteenth century. As in the eighteenth century, the often ignored effects and implications of these relations are of considerable value in illuminating East European history. The impact of the eighteenth-century "solution" of the Polish question on the creation of the nineteenth-century Ottoman question and Rumanian problem is nicely demonstrated. The importance of the Rumanian principalities as a conduit and staging area for the activities of the Polish emigration between 1792 and 1848 is thoroughly discussed. At the same time, the author shows the significance of the Polish exiles for

Rumania's own political awakening prior to 1848. This mutual benefit is shown to be the product of a number of similarities between Rumanian and Polish national, social, and geographical circumstances.

Overall, the author is successful in his attempt to describe and analyze Rumano-Polish relations in the eighteenth and nineteenth centuries. He highlights a number of important considerations that issue from his analysis, while avoiding distortion of either the local or European contexts. The link between the two main periods—1699 to 1792 and 1792 to 1848—is a little tenuous given the striking difference made by the partitions of Poland, but this does not greatly detract from the book. The author tends to assume the natural validity of Rumanian interests over those of the Polish leaders (which are fairly critiqued), but, overall, the book is remarkably free of special pleading. There is a useful concluding résumé in French that is largely a translation of the introduction.

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STELIAN POPESCU-BOTENI. *Relații între România și S.U.A. pînă în 1914* [Relations between Rumania and the U.S.A. up to 1914]. Foreword by CORNELIU BOGDAN. Summary in English. Cluj-Napoca: Editura Dacia. 1980. Pp. 228. 8.50 L.

Academic exchanges between the United States and Rumania have flourished in recent years, stimulated not only by the atmosphere of the 1970s in East-West relations and by Rumania's maverick foreign policy and the American desire to encourage it, but also by the natural longing of scholars in each country to move into what had been for more than twenty years a closed area. Consequently, American and Rumanian historians have been making visits for lectures and research, holding joint conferences, and celebrating notable anniversaries in the history of one or the other country. The year 1980 marked a century of diplomatic relations between the two countries (not counting years of World War II and the beginnings of the communist regime). The subject of American-Rumanian relations, accordingly, has had a natural attraction, especially on the Rumanian side. Visits to America were a welcome prize, and unused material in the U.S. archives was there for the picking. As long as the subject was far enough in the past and the necessary political genuflections were made, publication at home was no problem.

Stelian Popescu-Boteni's monograph, which covers those relations from the beginning up to World War I, is a worthy example of this genre. It is given official blessing by Corneliu Bogdan, former Rumanian ambassador in Washington, who in his preface stresses the contemporary relevance of America's historic interest in the independence of European

nations, and of Rumania in particular; at the end the author offers the expected encomiums to President Ceaușescu and to the sound principles on which he has based Rumania's relations with the United States. To have a historic tradition for good and constructive relations is a praiseworthy idea, but finding real substance in American-Rumanian relations before this century is no easy job. Popescu-Boten, who is a diligent and objective scholar, casts a wide net for material. He looked for everything he could find relating to Rumania in American sources, and vice versa, and put it all into his book.

The volume contains a bit of everything: passing references to the New World in Rumanian writings of the seventeenth and eighteenth centuries; reminiscences of stray travelers; the exploits of Rumanians who fought in the American Civil War; the beginnings of bilateral trade and the establishment of U.S. consulates in the principalities of Moldavia and Wallachia when they were still under Ottoman suzerainty; the wave of Rumanian immigration to America around the turn of the century; the occasional translation of American authors, from Benjamin Franklin to William James, into Rumanian; establishment of a Rumanian subsidiary in 1904 by Standard Oil; and statistics on the number of Rumanians owning Ford cars in 1912 (the answer is 107).

The author has ransacked the archives and libraries of both countries. He has culled data from Rumanian-American organizations. He has tracked down obscure memoir and press items. Much of this material is not without some interest as social or cultural history, but themes that might be developed, such as American literary influence in Rumania or the question of cultural assimilation of Rumanian immigrants in America, are not. The method, and to a large degree the subject, condemn the book to be what it is, a compilation of data for data's sake. The fact is that Rumania (or, before there was a Rumania, the Rumanian-inhabited principalities) held no political and very little economic interest for the United States government, and remote America was not important to the life and destiny of the Rumanians. That situation changed with World War I, in which more or less by accident the two states were on the same side and Wilsonian idealism found common ground with Rumanian aspirations to national unity.

This is only secondarily a history of international relations in the usual sense. Primarily it is a history of contacts that, even if dignified with a "people-to-people" label, were for the most part occasional, casual, and without very loud echoes in either nation.

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GEORGE EM. MARICA. *Studii de istoria și sociologia culturii române ardeleni din secolul al XIX-lea*. Volume

3, *George Barițiu—Istoric* [Studies in the History and Sociology of Transylvanian Rumanian Culture in the Nineteenth Century, vol. 3, George Barițiu—Historian]. Cluj-Napoca: Editura Dacia. 1980. Pp. 253. 14.50 L.

George Barițiu was one of the half-dozen most important Transylvanian Rumanians of the nineteenth century. His career, which spanned the half-century from the Vormärz to his death in 1893, is, in a sense, a history of the Transylvanian Rumanians, for there was no significant political or cultural undertaking of theirs in which he did not have a part. His occupations were diverse—journalist, politician, businessman, and educator—but all were ultimately directed toward the attainment of a single goal: national self-determination for the Rumanians of the Habsburg monarchy.

In the present volume George Em. Marica has chosen for investigation a neglected aspect of Barițiu's career—that of historian. He meticulously examines Barițiu's voluminous newspaper and journal articles, his publications of documents, and his major contribution to Transylvanian Rumanian historiography, *Părți alese din istoria Transilvaniei* (3 vols., 1889–1891), all against the general background of modern Rumanian cultural and intellectual development.

It is evident that Barițiu regarded history as an adjunct to his preoccupation with the national movement. Like so many of his colleagues elsewhere in Eastern Europe, he was convinced that the great political and social issues of the day could not be understood or resolved without a thorough knowledge of their historical background. Like his contemporaries, he also invoked historical arguments to support national political demands. Such a utilitarian conception of history explains the didactic, and sometimes elementary, character of much of his writing. Although Barițiu was not a professional historian, he nonetheless had immense respect for the craft, insisting that any serious work of history, including his own, must be grounded in the critical use of sources.

Marica makes clear Barițiu's devotion to the romantic-liberal tradition of European historiography, especially that represented by Michelet, Lamennais, and Guizot. Barițiu found their belief in progress particularly attractive, for it gave him hope that the Rumanians would eventually achieve the political and cultural level of other European peoples. Marica also surveys Transylvanian Rumanian historiography from the late eighteenth to early nineteenth century to show how strongly Barițiu was influenced by the native conception of history as an instrument in the service of enlightenment and the national cause.

A detailed analysis of Barițiu's writings reveals his almost boundless curiosity about the past and a

willingness to tackle any subject that would promote his political and social ideals. He made his most lasting contributions in articles on agrarian history, where he analyzed the persistence of "feudalism" in the nineteenth century, and in those works, like the third volume of *Părți alese*, where he drew mainly upon his own experiences. As both a journalist and a politician, he invariably wrote with one eye on the present and the other on the future. He was obviously most concerned with the history of Transylvania, but he also devoted much attention to Rumania, because, according to Marica, he realized that the fate of the Transylvanian Rumanians depended upon the welfare of the entire Rumanian nation. This is undoubtedly true, but Barițiu was also realistic enough to know that for the foreseeable future the destinies of the Transylvanian Rumanians would be determined in Vienna and Budapest, not Bucharest. He considered Austria a European necessity, and his works display a pro-Habsburg bias.

In this first comprehensive study of Barițiu as a historian, Marica has made an admirable contribution to the intellectual history of the Transylvanian Rumanians. Although it was not his primary concern, he has also enhanced our understanding of the classical national movements of nineteenth-century Eastern Europe.

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JOSEPH HELD, editor. *The Modernization of Agriculture: Rural Transformation in Hungary, 1848–1975*. (East European Monographs, number 63.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1980. Pp. 508. \$25.00.

The term "East European peasantry" conjures up images either of mud, squalor, and back-breaking hard work or picturesque villages and hamlets, men and women in colorful national costumes, copious meals, and lively festivals. The truth has always been a mixture of the two, tilting toward the prosaic grind of monotonous routine. The peasants were the backbone of East European societies, were frequently praised as such, and even more frequently were abused or ignored. Their pent-up anger flared occasionally, altering behavior that was usually submissive and alleviated only by a sense of cunning, their best weapon against landlords and city slickers in power.

The use of the past tense is justified, because the impact of technology, mobility, and governmental policies have undermined the exclusiveness of agricultural labor interwoven with a particular tradition-

al way of life, and the peasantry, as we have known it, is no more.

The fine essays in the book lead the reader through the progression from a Hungarian peasantry entering the modern age in a daze of suspicion brought about by decades of harsh and only partially successful accommodation up to the current dissolution of the peasant class as a distinct entity.

The emergence from serfdom and the battle between progress and tradition is well told by Antal Vörös, but the essay is incomplete. His emphasis on regional distinctions is valid, but it should have been rounded off by describing the stratification among the peasants within the villages and the different characteristics along ethnic and religious lines. World War I brought both hardship and gain to the peasants, and Peter Hidas's essay accurately mirrors this ambivalence. Béla Király explains clearly the nature of the peasant movements in the nineteenth and twentieth centuries. There is a small structural problem, easily correctable in a future edition: the story of István Nagyatádi Szabó's political career in the 1900s and 1910s should be treated together with the discussion on András Áchim, rather than in a chapter that follows the essay on the interwar period.

It is Király who summarizes the predicament of the Hungarian peasantry. "In fact," he writes, "never in the history of Hungary have the peasants really shared power, definitely never to the degree warranted by their numbers" (p. 345). Joseph Held's chapter on the interwar period is a dramatic illustration of this fact. His essay is thorough and well integrated; besides economic statistics and demographic data, the author depicts the character and consequences of stratification among the peasants and offers a vivid description of village life. Iván Völgyes's concluding chapter on the current dynamics of rural transformation is filled with personal impressions and is marked by a fortunate blend of scholarly insight and literary flair. One puts the book down with a healthy respect for what a mere hundred or hundred and thirty years can do and has, in fact, done to a social class, whose proverbial sturdiness and apparent immobility were so firmly anchored in our part-condescending, part-romantic urban imagination.

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MONIKA GLETTLER. *Pittsburg—Wien—Budapest: Programm und Praxis der Nationalitätenpolitik bei der Auswanderung der ungarischen Slowaken nach Amerika um 1900*. (Studien zur Geschichte der Österreichischen-Ungarischen Monarchie, number 19 or Schriften des DDR. Frank Josef Mayer-Gunthof-Fonds, num-

ber 13.) Vienna: Verlag der Österreichischen Akademie der Wissenschaften. 1980. Pp. 504. DM 100.

Monika Glettler, a West German who did her studies at, among other places, Vienna and Prague, carried out research in Budapest, and functioned recently as visiting professor at Columbia University in New York, possesses the prerequisites for her task. The study mainly examines the political interaction around the turn of the century between the Slovaks in the United States, those in upper Hungary (now the Slovak part of Czechoslovakia), and the authorities of the Habsburg monarchy with particular reference to Hungary—a subject largely overlooked so far by scholarly research. A cause of the gradual build-up of anti-Magyarism among the Slovaks from the 1890s is traced to a Hungarian measure through which Slovak clergymen were prohibited from going to America and taking over Slovak congregations. The positions thus left unfilled ultimately attracted Czech clergymen and, in due course, the seminaries in America became subject to Czech management. Czech-Slovak unity largely resulted from these circumstances and not from political agreements such as the one concluded in Pittsburgh (p. 270).

In any case, the Slovak peasants and their burgeoning intelligentsia, most of whom were only too willing in America and in Hungary alike to adapt themselves to the ways of the ruling culture (pp. 391–93, 399), did not see eye to eye. In any event, the “people about whom they spoke did not support them at all, nor did they understand them and they took notice of their appeals without any interest at all.” Nor did any social layer exist that would have embraced their ideas and spread them (p. 228).

The study is based on a very large collection of related archival material. Long and meticulous research is reflected in several hundred footnotes that represent information gathered from a large number of periodicals, including newspapers, as well as forty-three pages enumerating related general literature. Many documents referred to in the text reveal the long experience of the career diplomats of the Habsburg lands in ethnocultural matters. The documents often contain highly efficient and insightful assessments of the quickly changing social, economic, and political conditions affecting the immigrant groups of America at the turn of the century, in this instance, the Slovaks, and in some degree, the Czechs and Hungarians. They also warn of the dangers likely to develop in the context of the monarchy. The contrast between the accuracy of these assessments and the resigned inaction, which resulted from divergent forces in Austria-Hungary is striking. Thus, to a certain extent the Hungarian authorities appear in the role of Cassandra, always knowing the future, yet powerless to change the

course of destiny. In the words of the author: “the authorities in Budapest were considerably less consistent and energetic in the application of their nationality policies than ascribed to them in research so far. Also from this viewpoint, the usual post-[hoc] ergo propter [hoc] approach, with which the historiography of Austria-Hungary’s successor states tried to create an unduly simple causal relationship between the end of the First World War and the nationality problems of the Habsburg Monarchy, must be questioned” (p. 408).

These findings no doubt have been designed neither to reflect upon the sincerity of historians in the past who reached different conclusions nor to plead for a political cause. They seem rather to draw attention to the conflict between modernization and age-old loyalties at a feudalistic stage in which the unschooled peasant community was still strong enough not to be overly dependent for values on the city and its supracommunal “isms.”

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N. N. PUKHLOV. *Pol'skoe rabochee dvizhenie, 1890–1904 gg.* [The Polish Workers' Movement, 1890–1904]. Moscow: Nauka. 1977. Pp. 350. 1 r. 80 k.

In this study, N. N. Pukhlov investigates the development of the workers' movement in the three parts of partitioned Poland from 1890 to the end of the 1904–05 Revolution. The first chapter reviews the Polish workers' movement during the 1890s. The major topics discussed are: (1) the rapid industrialization that took place in the Polish Kingdom (Congress Poland); (2) the organization, program, and activities of the early Polish Socialist party (PPS); (3) the intensity of the struggle between those Polish Socialists—the so-called cosmopolitans—who supported certain alleged international proletarian interests and opposed nationalism, and those Polish Socialists who were reluctant to place the fate of their movement in the hands of the cosmopolitans who were mostly Russian revolutionaries. Supporting the cosmopolitans, Pukhlov argues that a union with the revolutionary forces in Russia was compatible in view of the Polish and Russian workers' common struggle against tsarist autocracy.

The last section of each of the three chapters examines matters relevant to the study in Galicia (Austria-Hungary) and the so-called western territories (Germany). Pukhlov notes that each area of partitioned Poland entered the era of capitalism under different conditions not only in the political but also in the economic and social sense. Galicia had the most favorable political possibilities but also the least advantageous economic conditions for industrial development. Nevertheless, a social-democratic movement developed there. The German (or

Prussian) portion of partitioned Poland, where the possibilities of political life were very restricted, drew benefit from belonging to a power whose economic development was extremely rapid.

The second and third chapters treat the struggle of the Polish proletariat at the beginning of the twentieth century and the state of the social-democratic movement in the Polish lands on the eve of the 1905 Revolution. Much space is given to the programs, activities, and factional conflicts of the various parties. The discussion relating to "the strengthening of the revolutionary links between the Polish and Russian proletariat" (p. 132) is extremely interesting. The Russo-Japanese War, the Revolution of 1905, and the problems resulting from these two events for Polish Socialists and for the newly organized Polish political parties are fully examined.

Pukhlov's study, despite its overbearing Marxist-Leninist bias, is a good and useful book. No brief review can do justice to its various interpretations of historical phenomena. Anyone interested in the development of the workers' movement in Poland and able to read Russian will find the book exasperatingly interesting.

The volume contains an extensive bibliography of Polish and Russian works but not a single English title. Two items bearing the name L. I. Brezhnev, one item bearing the name M. A. Suslov, and three items bearing the name Edward Gierek are listed, but the titles are unrelated to the subject of Pukhlov's study. An appendix containing documents, party programs, minutes of meetings of congresses, party press editorials, and so forth is also included. An important blemish is the absence of an index.

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ROMAN WAPIŃSKI. *Narodowa demokracja, 1893–1939: Ze studiów nad dziejami myśli nacjonalistycznej* [National Democracy, 1893–1939: From the Study of the History of Nationalistic Thought]. Summary in English. Wrocław: Ossolineum. 1980. Pp. 337. 70 Zł.

Without doubt, the national democratic movement has been one of the dominant currents in Polish sociopolitical life from its inception in 1893 through the interwar period and, one could argue, in spirit to the present. Although its importance has attracted the attention of many scholars and commentators, Roman Wapiński has produced the first study of this highly controversial subject that is as objective as possible, given the bias inherent in the source material. At the outset, he states that his book will focus on the evolution of nationalist political thought and ideology, rather than simply recounting the formation of an organizational structure or the various activities of the leading personalities. In

the process, Wapiński isolates several major themes that have persisted largely unchanged in national democratic doctrine: an extreme form of nationalism that emphasized a spiritual bonding leading to the unification of the Polish nation; a concept of "nation-state" in which the former ethnic component is clearly superior to the latter political portion of this equation; a close identification between Polish and Latin culture, which was considered to be synonymous with the Roman Catholic faith, and the subsequent ethnoreligious exclusivity that refused to admit non-Polish Catholics to privileged membership in the nation; a hostile attitude to any movement whose doctrine featured a supranational, universal focus, especially socialism; finally, a conviction that the main danger to the Polish nation came from Germany rather than Russia, hence a concentration of intellectual and organizational efforts aimed against Germany prior to 1917 and against Soviet Russia thereafter.

In outlining these main themes, Wapiński highlights the social, economic, political, and intellectual circumstances that combined to form the perfect environment for a movement of this type. He underscores the fact that national democracy attracted followers from among all social strata, although its main base of support rested in the emerging Polish middle class. Accordingly, he views the movement's antisemitism as essentially an expression of the socioeconomic frustration experienced by the middle class upon encountering competition from the solidly entrenched Jewish merchants, artisans, and professional people. He also traces the changes in tenor and emphasis in nationalist ideology as the Poles fought for the establishment of an independent state, and then struggled until 1926 to find a viable political system, only to have Józef Piłsudski—the archenemy of the national democrats—seize power in a successful coup d'état. As younger, less patient elements began to surface in the leadership ranks of national democracy, its ideology split into two divergent streams, one of which became increasingly similar to doctrines then current in Italy and Germany. Although this proto-fascist strain of national democracy was not representative of the movement as a whole, it did cast the movement in an unfavorable light both inside and outside Poland.

In the process of his presentation, the author delivers far more than promised in the introduction. He does, in fact, paint a detailed picture of the changes in organization and leadership personnel that occurred throughout the period and carefully integrates his portrait of national democracy with the panorama of other Polish sociopolitical movements and the national scene. Although the book lacks a subject index and a bibliography, the notes are quite detailed and reveal an impressive research

base. Perhaps most important, the general tone of the book is very objective, as professional and uninvolved as possible—a tendency heretofore absent from studies of national democracy by Polish scholars, irrespective of residence. This work should stand alone as the definitive word on its subject for considerable time to come.

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IA. N. SHCHAPOV. *Vizantiiskoe i iuzhnoslavianskoe pravovoe nasledie na Rusi v XI–XIII vv.* [The Byzantine and South Slavic Legal Heritage in Rus in the Eleventh to Thirteenth Centuries]. Moscow: Nauka. 1978. Pp. 291. 2 r. 30 k.

Ia. N. Shchapov, a protégé of the late A. A. Zimin, is proving to be one of the best of Soviet medievalists. In numerous articles and two books published over the last twenty years, Shchapov has delved deeply and extensively into the manuscript tradition of early Rus. His work on the *ustavy* (statutes) attributed to Vladimir and Iaroslav has shown him to be a skilled textologist, and in the present work, only partially revealed in his previous articles, he turns his considerable skills to the textual history of the *Kormchaia kniga*, or nomocanon, in Rus from the eleventh through the thirteenth centuries.

As the principal ecclesiastical law code, the *Kormchaia kniga* offered Byzantine (and to a lesser extent South Slavic) church laws, decrees, canons, teachings, and the like in Slavic translation, and in Rus the *Kormchaia* was reworked and added to according to the views, the location, the knowledge, and the skills (and deficiencies) of various editors and scribes who produced *Kormchaia* copies, nearly two hundred of which survive. Since each copy is about three or four hundred folios long, and the articles added in Rus number as many as forty, the task of studying this mammoth body of material is a challenge indeed. Prerevolutionary Russian scholars studied and published portions of the *Kormchaia*, but their interests were typically narrowly religious and their knowledge of manuscript copies limited. Soviet scholars have tended to use *Kormchaia* copies for their more secular contents, either Byzantine (for example, excerpts from the *Ecloga* and *Prochiron*) or Rus (*Russkaia pravda* and Vladimir's and Iaroslav's *ustavy*). Shchapov is the first to bring modern textological methods to the study of essentially all *Kormchaia* copies and of their entire contents, whether secular or ecclesiastical (categories which in any case tend to break down, as in the classification of civil laws on marriage and family relations).

Few scholars, I suspect, will want to read all of Shchapov's chapters 2, 3, and 4, where he analyzes *Kormchaia* redactions, recensions, manuscript gene-

alogy, and textual history in painstaking detail. All scholars of Rus, however, would profit from reading his introduction (pp. 3–12), where he examines the usefulness of studying the *Kormchaia*, his chapter 1 (pp. 13–39), in which he summarizes previous historiography and outlines the methodology of his *Kormchaia* research, and his concluding section (pp. 234–54), in which he explores the historical significance of several of his major textological findings. Many will find that chapters 2 through 4 offer important information on several Rus documents, such as the *Russkaia pravda*, although the absence of a subject index—a common deficiency of Soviet monographs—makes it difficult to find all relevant citations. Indexes of personal and geographical names and of manuscript copies are given. Those interested in specific copies will find the addendum (pp. 255–75) useful, where he provides careful descriptions of fifty-three of the more important copies.

Shchapov's work is not finished. He plans (p. 33), for example, to publish an edition of the *Kormchaia* to supplement V. N. Beneshevich's partial *Kormchaia* edition of 1906–07. Meanwhile, he is proving that textological analysis of early Rus manuscripts can open new doors to historical research.

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A. IA. DEGTIAREV. *Russkaia derevnia v XV–XVII vekakh: Ocherki istorii sel'skogo rasseleniia* [The Russian Village in the Fifteenth to Seventeenth Centuries: Essays on the History of Rural Settlement]. Leningrad: Izdatel'stvo Leningradskogo Universiteta. 1980. Pp. 175. 1 r. 80 k.

In the last two decades there have been a number of major studies of the Russian countryside. The analysis of the Novgorodian land cadastres directed by A. L. Shapiro has been a particularly significant contribution. A. Ia. Degtiarev attempts to synthesize this recent scholarship, and thus this work represents a good introduction to the current state of Soviet historiography on sixteenth- and seventeenth-century agrarian history.

After presenting a historical sketch of rural settlements in which a formula for a coefficient of rural density is established, Degtiarev analyzes village settlements in northwestern Russia from the late fifteenth through the first half of the sixteenth century, and then from the last third of the sixteenth century through the seventeenth century. Chapter 4 is a perfunctory discussion of the central regions, and chapters 5 and 6 are devoted to the impact of "feudal" rents on the village and the distribution of poor, average, and good lands (*Great sokha*).

At the end of the fifteenth century Novgorod was

pockmarked by approximately thirty-eight thousand small settlements. Ninety percent of these villages comprised fewer than five *dvora* and 70 percent contained fewer than one or two *dvora*—a pattern that was retained until the middle of the sixteenth century and that also reflected the rural settlements of the central regions. The 1548 land cadastre of Tver shows the average settlement to be 2.05 *dvora*, while those of the Troitse-Sergiev monastery averaged between one and three *dvora*.

One interesting but controversial trend is the high percentage of slave *dvora* on arable land. Already by the mid-fifteenth century 50 percent of the *dvora* on large lay estates in Bezhetskaia *piatina* were housed by slaves. Along with slave *dvora* there was from the beginning of the sixteenth century a continuing growth of rural *bobyli*, who together with the slaves reflected the general debasement of the peasantry into serfdom.

Degtiarev amply demonstrates the economic crisis in the last third of the sixteenth century and the Time of Troubles. The devastation of the rural economy wrought by the *oprichnina*, war, famine, plague, and high taxation is a picture familiar to most scholars. Peasant flight and death caused massive depopulation and the abandonment of arable lands in large areas of the countryside, which in turn intensified the competition between ecclesiastical, boyar, and *pomest'e* landholders for peasant labor. In Novgorod Bezhetskaia *piatina* lost two-fifths of its population and as late as the 1646–47 cadastre the number of peasants per square kilometer was less than ten times what it had been a hundred years earlier. In Derevskaia *piatina* 93.5 percent of the arable land was abandoned, and the depopulation reached the astronomical proportion of 92.9 percent. Degtiarev's tables for the central regions between the cadastres of 1573 and 1597 show an average of 48 percent for abandoned arable lands, and although the samples used are often statistically small, the general trend of economic disaster is clear.

Degtiarev also reviews the arguments over whether the transformation of peasant rents around 1500 from payment in kind to money represented a real increase in the tax burden. Basically he believes that peasant life deteriorated only from the 1570s, and it was in these latter decades of the sixteenth century that the character of the village fundamentally changed.

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JOSEPH T. FUHRMANN. *Tsar Alexis: His Reign and His Russia*. Gulf Breeze, Fla.: Academic International Press. 1981. Pp. viii, 250.

The use of a notable individual as the focal point for studying a given era is a time-honored historio-

graphical convention. Joseph T. Fuhrmann's book concentrates for the first time in English on a lesser-known but pivotal figure in Russian history.

The reign of Tsar Alexis (1645–76) witnessed the consolidation of the Muscovite state. Although this was a process of significance in its own right, it also preceded the impact of the westernizing innovations of Peter the Great in the first quarter of the eighteenth century. If one wishes to settle upon an informed opinion whether those innovations "turned Russia in a new direction" (according to one view) or whether they "accelerated a preexisting and ongoing process" (according to another) or whether they "merely covered an essentially Oriental despotism with a superficial patina of Western culture" (according to still a third), a knowledge of Tsar Alexis and his Russia is indispensable.

Fuhrmann's book can be a helpful introduction to the complexities of the period and of the man who ruled as tsar. To overcome the complexities, Fuhrmann has diversified his manner of approach. Of the three parts of his book, the first and third are predominantly chronological, bracketing together the years from approximately 1625 to 1698. They are divided by the year 1656, which marked for Alexis the end of a successful series of campaigns against Poland-Lithuania and which ushered in the mature and more self-assertive phase of his rule. The second, or middle, part is primarily topical. It deals with such matters as the ordinary and ceremonial life of the tsar and tsarina, the variety and function of governmental offices, the types and organization of military forces, and the conduct of commercial activity. By utilizing this format, Fuhrmann is able to survey the personal life and character of Tsar Alexis and to touch on all the major aspects of his reign.

Shortcomings there are, but it is not the purpose here to provide an errata list. By way of suggestion, an extremely valuable addition to the bibliography (since Russian titles are included in the 140-odd entries) would be *Malorossiiskoe vlianie na velikorusskuiu tserkovnuiu zhizn'* by K. V. Kharlampovich. It is a treasury of information on cultural and church history. It is also interspersed with some intriguing interpretations. For example, microscopic analysis of the arrival and departure of individual foreigners led Kharlampovich to conclude that the existence of the famous school (p. 38 in Fuhrmann) at the site of the monastery founded by F. Rtishchev is a fiction. To my knowledge this assertion, important for the history of education in Russia, has yet to elicit notice or response.

Fuhrmann's biography of Tsar Alexis tries to be both scholarly and popular. It is. These aspects, however, are compartmentalized. The writing is at times anecdotal, at others, encyclopedic: beneath the flow of exposition can be detected the differing textures of the source materials. While this does not

detract from the value of the book for the objective information it contains, it does diminish the level of integration in the treatment of its chief subject. The personality of Tsar Alexis peers through occasionally, but it still remains imprisoned in the sources.

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A. P. KORELIN. *Dvorianstvo v poreformennoi Rossii, 1861–1904 gg.: Sostav, chislennost', korporativnaia organizatsiia* [The *Dvorianstvo* in Reformed Russia, 1861–1904: Composition, Number, and Corporate Organization]. Moscow: Nauka. 1979. Pp. 303. 2 r. 30 k.

This is at once a very valuable and a disappointing book: valuable, because it presents in a coherent manner a great mass of material on the socioeconomic and political characteristics of imperial Russia's first estate during the country's transition from a peculiar sort of society of orders toward a modern class society; disappointing because, in sum, it adds nothing of substance to the pioneering descriptive-structural studies published by the same author about a decade ago in the leading Soviet historical periodicals. These studies, especially the nearly book-length work published in *Istoricheskie zapiski* (which bore the same title as the present book), can be found cited in the footnotes of virtually any recent study dealing with Russian social history of the late imperial period; they have become standard reference works.

A. P. Korelin's aim in preparing the book under review was evidently to transform his earlier work into a proper monograph, a book with a thesis (presumably in order to present it for the doctoral degree). With all respect for Korelin's erudition and gratitude for his excellent reference work, I cannot say he has succeeded in this latter enterprise. The book is, to be sure, not simply a padded republication of the earlier articles: they have been reworked, partly to take account of subsequent scholarship (which the author scrupulously acknowledges, at least so far as Soviet scholarship is concerned) and partly to add an evaluative framework to the descriptive material. There is also some new material, including several fascinating statistical tables found by the author in the archives of the imperial bureaucracy. Moreover, there are several new subchapters (on noble entrepreneurship, the nobility and the *zemstva*, and the marshals of nobility), and an entirely new final chapter (on the political activity of the landed nobility from the 1860s to the end of the century). These new pages add nothing of substance, however, to the published work of other scholars on the noble component in the bureaucracy, government policy toward the nobility, and noble (landed gentry) opinion in the second half of

the nineteenth century (the principal names here are P. A. Zaionchkovskii, L. G. Zakharova, Iu. B. Solov'ev, and V. G. Chernukha). Korelin's interpretive comments conform to the more nuanced treatment of the problem of social forces in late imperial Russia now current in Soviet historiography, but they add nothing at all to it; quotations from Lenin and references to "the objective laws of capitalist development" are all too often substituted for serious historical analysis.

In short, we do not have here the kind of serious attempt to assess the political role of the nobility through close analysis of government decision making that has been employed with considerable success by such scholars as Chernukha and Solov'ev for the period treated in this book, or by V. S. Diakin, L. H. Haimson, and others for the post-1905 years.

Finally, it must be regretted that, in view of the essentially reference-work character of Korelin's contribution, he has not carried his description of the legal, social, and economic status of the nobility through to 1917.

All the preceding notwithstanding, there is no question but that Korelin has made an important and enduring contribution to historical scholarship on imperial Russia.

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REX REXHEUSER. *Dumawahlen und lokale Gesellschaft: Studien zur Sozialgeschichte der russischen Rechten vor 1917*. (Beiträge zur Geschichte Osteuropas, number 12.) Cologne: Böhlau. 1980. Pp. vii, 254. DM 68.

Students of imperial Russia have tended to stress the overriding role of the central government, overlooking the impact of local political developments upon national politics and policy making. Rex Rexheuser's *Dumawahlen und lokale Gesellschaft* represents a pioneering effort to remedy this deficiency for the late imperial period by combining an analysis of the elections to the four State Dumas of 1906–17 with a detailed account of political developments within Kursk province, particularly among the local landowning gentry. The province selected for Rexheuser's local study, although somewhat atypical as the author himself readily recognizes, is most aptly chosen. Kursk was the leading bastion of the post-1905 gentry reaction, which played a key role in shaping the political options of the late tsarist regime. It was also the home province of both Nikita Khrushchev and the Brezhnev family, so that the events described in this book continue to exert an influence upon Russian political life until this very day.

Although well conceived, Rexheuser's study falls short of the expectations of specialists who are versed in the latest American work on this period,

particularly the rash of literature on gentry and Duma politics currently in press or nearing completion. Unlike these American studies, which are based on extensive research in Soviet archives and libraries, Rexheuser limits himself to an exhaustive analysis of published sources readily available in the West. His discussion of the Duma elections therefore is necessarily limited to aggregate election results, without probing in any detail alignments within the provincial electoral assemblies or utilizing the biographical data on the Duma electors that American specialists have found so useful in their ongoing studies of this period. Consequently, deep-seated differences in the political behavior of the various electoral curiae—and hence among the conflicting classes of Russian society, particularly its nongentry elements—remain largely unexplored. The sharp move to the right after 1907 is explained in terms of local developments, in the main the recent political organization and mobilization of the gentry right. The impact of the restructured Duma election law of June 3, 1907, which greatly enhanced the political influence of the larger gentry landowners, fails to receive the attention that it deserves. Yet without a change in the Duma franchise in the gentry's behalf, the newfound organizational abilities of the gentry right proved to be of little avail, as the elections to the Second State Duma demonstrated.

Despite these shortcomings, this study represents a pioneering, often perceptive attempt to explore the local roots of national political developments. It also offers a first-rate account of *zemstvo* politics in Kursk from the late nineteenth century to the outbreak of the First World War. Until studies based on a broader range of sources become readily available in print, *Dumawahlen und lokale Gesellschaft* deserves a careful reading from all those interested in the underlying causes of the fall of the old order in Russia.

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S. M. SAMBUK. *Politika tsarizma v Belorussii vo vtoroi polovine XIX veka* [Tsarist Policy in Belorussia in the Second Half of the Nineteenth Century]. Edited by V. P. PANIUTICH. Minsk: Nauka i Tekhnika. 1980. Pp. 221. 1 r. 10 k.

This slim study by the Belorussian historian S. M. Sambuk offers more than its size would otherwise indicate. Its almost exclusive archival base and only minimally overbearing ritualistic quotations of Marx's and especially Lenin's pronouncements on subjects without connection to Belorussian history make this study appreciated. It contributes to a better understanding of tsarist policy in Belorussia after the second Polish insurrection of 1863. Quite

symbolic of the present situation in the USSR is the fact that this Belorussian historian has written a study sponsored by the Belorussian Academy of Sciences and published in Belorussia in the Russian language. This same situation prevailed in fact in Belorussia under the tsarist regime.

The narrative of the work revolves around three main themes: the tsarist autocracy and the nobility, the autocracy and the peasantry, and the tsarist nationality policy of de-Polonization of Belorussia followed by implementation of a program of Russification of Belorussian society. Making the best of the defeat of the rebellious Poles, Russia initiated a twofold policy aimed at the forceful resettlement of the Polish landowning nobility in Siberia and distribution of their estates among Russian and Baltic German gentry who were brought to Belorussia. The newcomers, in addition to numerous Russian state officials dispatched there en masse, were looked upon as the loyal pillars of the regime. As before, the Belorussians were left out, exposed to Russification and economically suffering a colonial-type exploitation.

Sambuk provides extensive data on all aspects of tsarist policy, including numbers on the resettled Poles, the new Russian colonizers, the nobility, and officials, in addition to information on the demographic composition of the landowning and landless nobility and the peasantry in all four categories—state peasants, freeholders, tenants, and Old Believers. In order to weaken the status of the Poles the regime declassified 46.6 percent of the landless gentry to ordinary peasants, exiled 12,355 Poles to Siberia and confiscated their property, prohibited the use of the Polish language in official places, and Russified educational institutions in language and program to eliminate the "Polish spirit" and replace it with the "Russian spirit formulated as Orthodoxy, Autocracy, and Nationalism" (p. 51).

Analyzing the Russian agrarian and social policy in Belorussia, Sambuk explains its failure as the regime's inability to eliminate the remnants of the feudalistic form and to create a modern capitalistic system. By sacrificing the economic interest of the peasantry to win the loyalty of the Russian landowners (that is, making peasant households economically unproductive and forcing them into various forms of bondage), the state caused the western territories of the Russian empire to remain more backward than central Russia. As a consequence, "tsarism failed to remove contradictions in its agrarian policy during the four decades after the 1861/63 reforms" (p. 199).

Yet the greatest losers were the Belorussians who, while witnessing the exchange of their oppressors, did not progress at all on economic or on national-cultural levels.

Apart from some Marxist-style generalizations,

the study provides a wealth of information that can be well used by the Western student of Belorussia, a much-neglected subject not only in the West but also in the Soviet Union.

STEPHAN M. HORAK
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V. IA. GROSUL. *Revolutsionnaia Rossiia i Balkany, 1874–1883* [Revolutionary Russia and the Balkans, 1874–83]. Moscow: Nauka. 1980. Pp. 333. 2 r. 40 k.

The many Western writers who have dealt with aspects of the Eastern Question have tended to emphasize diplomatic and political themes with a stress on the relations among the great powers and between these states and the Balkan nationalities. Soviet and socialist bloc writers, although also producing primarily standard diplomatic histories, have also shown an interest in social and national revolutionary activity and the support or opposition that individual movements found in Russia. The author of the present study, V. Ia. Grosul, has written a previous work, *Rossiiskie revoliutsionery v Yugo-Vostochnoi Evrope, 1859–1874* (1973). The book under review continues that work, covering the years of the Eastern crisis of 1875–78 and concluding with the assassination of Alexander II. The revolutionary movement in Russia during this period is discussed, as well as its members' attitude to and participation in the major Balkan conflicts—the Bosnian uprising, the war between the Ottoman empire and the Balkan states of Serbia and Montenegro, and the Russo-Turkish War of 1877–78. Events in Rumania, which was the center of the activities of the Russian revolutionary emigration, are also described.

In the 1870s, a decade during which official Russia concerned itself deeply with Balkan national revolutions, the tsarist government was itself an object of attack by revolutionary movements whose members were also active in the Balkans. The book primarily describes the position of the *narodniki* both in Russia and in the emigration. Thus the attitudes of followers of Bakunin, Lavrov, and Tkachev and the views of their newspapers *Rabotnik*, *Vpered*, and *Nabat* receive particular attention. Although their opinions on the immediate issues differed, the majority disapproved of Russian intervention in the Balkans. They strongly supported the idea of national liberation but disputed the ability of the Russian autocracy to accomplish the task. They also believed that the revolutionary movement should focus its weak resources on preparing Russia for revolt. Intervention in the Balkans would simply distract the youth from the internal struggle. The revolutionaries were also closely associated with national organizations, in particular those of the Poles and Ukrainians, whose animosities were di-

rected against St. Petersburg and not against Constantinople.

Despite the negative attitude of the leadership, some members of the revolutionary movement did, nevertheless, participate in the Balkan events. Their numbers were, however, extremely small. About thirty to forty joined in the rebellion in Bosnia and Hercegovina. Of the five thousand Russian volunteers who went to Serbia, only about one hundred represented the revolutionaries. Two hundred of the six hundred thousand Russians involved in the Russo-Turkish War of 1877–78 were engaged in such activities. The author is critical of the failure of the revolutionary circles to understand the "objectively progressive" character of the Russian military effort.

In addition to these questions, the book discusses in detail the importance of Rumania as a transit area for the importation of illegal literature into Russia, the activities of the revolutionaries in the Russian army, and the upswing of radical action after the war, culminating in the assassination of the tsar. The major contribution of this study is its detailed description of the involvement of individual revolutionaries in Balkan events and its reflection of the views of current Soviet historiography on the activities of these socialist circles.

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ANDREW ROSSOS. *Russia and the Balkans: Inter-Balkan Rivalries and Russian Foreign Policy, 1908–1914*. Buffalo: University of Toronto Press. 1981. Pp. xiii, 313. \$35.00.

E. C. Helmreich published the standard work on the background and diplomacy of the Balkan wars in 1938. His study, which presented the Balkan events of 1908–14 in the perspective of European diplomatic history, included a perceptive chapter on Russian foreign policy in the Balkans. Between 1938 and 1940 Soviet scholars brought out six volumes of tsarist diplomatic documents for the period 1911–12. These volumes provided the major fresh source for a monograph I wrote in the 1960s on Russia and the Balkan alliance of 1912. In 1970 Mieczysław Tanty made effective use of Bulgarian diplomatic archives to study in detail Russian Balkan policy during 1912 and 1913 (*Rosja wobec wojen bałkańskich 1912–1913 roku*).

Rossos, Helmreich, Tanty, and others provide balanced, scholarly narratives that cover in some detail the diplomatic history of the Balkans during the years 1908–14. Helmreich, who worked in the Austrian archives in addition to thoroughly combing the available European, Soviet, and Balkan diplomatic documents and secondary materials, commented particularly persuasively concerning

the motives of Austrian policy in the Balkans. He also offered a well-documented and careful discussion of the main considerations that underlay the foreign policies of not only the major European states but also those of the smaller Balkan powers.

Rossos complements Helmreich by using new Russian and Balkan materials and by viewing the diplomatic events of the peninsula during the years 1908–14 from the point of view of Athens, Belgrade, and Sofia rather than from that of Vienna and the other European capitals. He has worked with materials contained in the Michel de Giers and Serge Botkin archives at the Hoover Institution on War, Revolution and Peace. These archives, consisting of lithographic copies of correspondence between the foreign office in St. Petersburg and its representatives at the Russian mission in Darmstadt, do not, however, take the place of a systematic, scholarly edition of documents from the tsarist Russian archives.

Lacking a systematic collection of Russian documents for the period 1908–14, Rossos is not in a position to discuss in detail the formulation and conduct of Russian foreign policy during the entire period examined in his study. He is frequently obliged to rely on fragmentary diplomatic correspondence and the subjective views of Balkan and European diplomats for his comments on Russian Balkan policy. Here he does not seem to probe very deeply, and his characterization of Russian policy differs little from the views of pre-1914 Balkan, French, and British diplomats and from what German historian Otto Bickel wrote on Russian Balkan policy in the monograph *Russland und die Entstehung des Balkanbundes 1912*, a dissertation published in 1933. But Rossos does use materials not consulted by other Western historians, especially the Giers-Botkin papers and a number of important Bulgarian, Greek, and Serbian secondary works. He offers insights into the Balkan rivalries of the pre-1914 years and, in this way, contributes to our understanding of the events that led to the outbreak of World War I.

Western scholars have yet to gain access to Soviet and Balkan diplomatic archives for the period 1908–14. Even on the basis of the materials now available, it would seem possible to write still another book on Russia and the Balkans on the eve of World War I. B. H. Sumner did not have access to Balkan and Soviet diplomatic archives when he wrote his classic work on Russian Balkan diplomacy during an earlier period, the decade of the 1870s. What is needed for the pre-World War I period is a work that will make foreign policy an integral part of the internal political, economic, social, and intellectual history of Russia and of the peoples of the Balkans.

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NEIL HARDING. *Lenin's Political Thought*. Volume 2, *Theory and Practice in the Socialist Revolution*. New York: St. Martin's Press. 1981. Pp. 387. \$32.50.

Neil Harding's first volume, covering Lenin's political thought up to 1914, was favorably reviewed by Ronald Gregor Suny in the October 1980 issue of the *AHR* (pp. 947–48). I agree entirely with the evaluations in that capable review, many of which apply to the second volume as well, and thus I will avoid, insofar as possible, repeating points made in it.

The underlying argument of both volumes is that Lenin was a remarkably consistent and coherent Marxist; he was most emphatically not a mere power-hungry politician who contorted Marxist theory to fit shifting political opportunities. Even less justifiable is the related contention that Lenin was some sort of Blanquist with Tartar sauce; he owed incomparably more to Marx, Engels, Kautsky, and Plekhanov than to Tkachev or other *narodniki*.

This does not mean, as some might rush to conclude, that Harding is an uncritical admirer of Lenin. Rather, he has taken a fresh and careful look at Lenin's writings and has concluded that many influential scholars have not done Lenin justice. Indeed, the criticisms that Harding does make of Lenin are all the more telling because one does not sense in either of his two volumes the animus against Lenin and Bolshevism that at times so colors the works of Ulam, Pipes, or Schapiro.

To the casual observer, at least, the case for Lenin's fidelity to Marxism may seem inherently more problematic in the second volume, which covers 1917, when Lenin entered with such gusto into the maelstrom of political action, than in the first volume, which discussed a period when isolation and political impotence made ideological purity easier to maintain. But Harding carefully and convincingly demonstrates how Lenin's momentous move from agitation for a radical-democratic revolution in prewar Russia to a belief in the necessity of world revolution had to do directly with his analysis of capitalism rather than with immediately perceived revolutionary opportunities. Similarly, Lenin's "anarchist" slogans and measures in 1917, although ostensibly opportunistic—and certainly a decisive element in Bolshevik popularity—nevertheless fit neatly into Lenin's prerevolutionary theoretical formulations, which in turn were firmly based on Marx's own writings. (To be sure, the ambiguity of Marx's and Engels's writings on these points left Lenin the possibility of moving from the libertarian "commune state" of late 1917 to the ever more despotic dictatorship of the proletariat and party elite after early 1918, but Harding argues persuasively that this was not a premeditated move by Lenin; he was forced into it by harsh reality.)

Lenin was quite capable of backing off when it

became clear that a particular policy, whatever its basis in Marxist theory, was not working. To recognize this—and Harding does—introduces an important qualification to the assertion that Lenin was a consistent Marxist. In backing off, Lenin did usually try to find other Marxist texts to support new actions, but he was not always successful—which again Harding recognizes. Indeed, the concluding sections of volume 2 painstakingly document the deep tragedy of Lenin's last years, and of his life's work, as his revolution and party failed to live up to his expectations. It was at *this* point that Lenin lapsed into a kind of Blanquism or Jacobinism.

If I were to find fault in what I consider on the whole to be a most admirable study, I would concentrate on Harding's insufficient attention to Lenin's repeated inclination to back off from ideologically derived positions. I missed, for example, any discussion of Brest-Litovsk, or of Lenin's zigzags in the summer of 1917 on the issue of the proper role of the soviets. It might be replied in Harding's defense that he could not cover everything in a study that already extends beyond 600 pages. Yet much more could have been covered had the two volumes not been so loosely composed, with far too much direct quotation. In volume 2 Lenin's own words constitute close to 50 percent of the text. With more economical writing many more examples could have been considered and, presumably, a more richly textured picture presented.

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MICHAEL KETTLE. *Russia and the Allies, 1917–1920*. Volume 1, *The Allies and the Russian Collapse, March 1917–March 1918*. Minneapolis: University of Minnesota Press. 1981. Pp. 287. \$27.50.

The dutiful reviewer keeps plodding through this book assuming, given that it was published by a reputable university press, that there must be some merit that will show up later (not having done so sooner). Dutiful reviewer assumes wrongly. First of all, Michael Kettle's book is *not* about the Allies and the Russian collapse. At best it is about Britain: France, the United States, and Italy do not exist for this study except in the last chapter. In fact, it is not even about Britain, but rather about a small group in the cabinet, the War Office, and the embassy and other missions in Russia. The debates and controversies that wracked British opinion, especially the Labour party, are missing. Even if accepted as an account of how a small but important circle watched the collapse of Russia, what we have are a lot of telegrams and cabinet discussions devoid of integration into the broader strategies, policies, or problems of Britain or Russia.

If the other Allies are missing, the enemy is not, for Kettle does break out of his narrow shell periodi-

cally to pursue the subject of Parvus-Helphand and German machinations. Their role in the Russian Revolution is a secondary theme, largely divorced from the main topic but occasionally injected into the book for a few pages, and grossly exaggerated. For example, historians of the Russian Revolution will be surprised to learn that the Kronstadt sailors and the machine gunners in the July Days were actually stirred up by German agents and that their opposition to the offensive was only "alleged"! To show the importance of Helphand in Kettle's view of things, he gets over one-third of the space devoted to "The Origins of Revolution."

Kettle's knowledge of Russian history and use of sources is appalling. Although General Kornilov is the central figure in two of his chapters, Kettle has not used the major sources of materials or serious studies by Western, much less Soviet, historians, on the Kornilov affair, and he seems innocent of any understanding of the issues in and controversies about the Kornilov affair and its supporters. Indeed, Kettle uses almost none of the extensive materials available for his entire subject and seems to be proud of it. The book is based almost entirely on cabinet and War Office records, plus a small handful of other sources. Even the Foreign Office records are used only in small part. Thus, even as an account of official British reaction the book fails because of the inadequate source utilization. The end result is an extremely narrow focus, itself incompletely researched, plus an incredibly naive and ill-informed version of what was happening in Russia and Britain.

What purpose is served by this tedious combination of myopic use of cabinet and War Office records with a gossip-column style of writing? Is anyone supposed to be surprised to learn that British officials had only a poor grasp of what was happening in Russia and less ability to control it? Do we really profit from the "X, nephew of Y, soon to be Lord Z," style of writing, from a book that breathlessly tells us that a minor British functionary in Russia in 1917 was a cousin of the novelist Doris Lessing? The book may have some virtue as a guide to what is in the British War Office and cabinet records pertaining to Russia, but only in part. Moreover, given the odd themes that the author seizes upon and blows out of proportion (the role of Helphand; British interest in control of Russian banks, which he makes into the main foreign policy objective; an obsession with who was a Jew, and so forth), the reader is not entirely convinced of its reliability as a guide. What is in many ways most depressing about the book is the statement that four more volumes are forthcoming, apparently already written.

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TIMOTHY DUNMORE. *The Stalinist Command Economy: The Soviet State Apparatus and Economic Policy, 1945–53*. New York: St. Martin's Press. 1980. Pp. xi, 176. \$22.50.

Timothy Dunmore advances the proposition that Stalinism cannot be absolute even under Stalin himself. This insight is supported by his analysis of the institutional patterns of the Soviet command economy between 1945 and 1953. While admitting the peculiar circumstances of postwar reconstruction under the aging dictator, the author shows how a bureaucracy in a complex imperial economy can exercise autonomous power.

Dunmore points out substantive disparities between the policies announced by the command center (that is, the Politburo headed by the First Secretary) and their implementation by the ministries. He demonstrates how the lower strata of the command pyramid could influence the higher ones and how factionalism within the Politburo could be exploited by the dictator and the ministries.

In support of his thesis, the author analyzes two issues. First, he reviews the problem of balance between heavy and light industries. The Politburo under Stalin proclaimed in the fourth five-year plan its intention to give greater priority to light industry so as to benefit the consumer. Under the entrenched sectoral rather than territorial administration of the economy, however, the powerful ministries of machine building, metals, and transport managed to frustrate the new priority.

Second, the author investigates the relation between the Politburo's announced policies and the process of regional economic development of the USSR. At issue was the extent of favoritism of the vast eastern regions at the expense of the "Old West"; the latter, incorporating the war-damaged Ukraine and Belorussia as well as the newly annexed Baltic states, was promised by the fourth five-year plan greater priority than was actually implemented by the ministries.

One would agree, therefore, that the command system was less than absolute. This explains why and how Khrushchev's attempt to reform the economy in 1957 according to a regional principle was frustrated by the branch-oriented bureaucracy. This reviewer, however, would disagree with the author that the label "command economy" as applied to the period in question needs to be softened with appropriate qualifiers. Without the market, the system was extremely centralized, and Stalinism meant extensive threats and compulsion. While Dunmore avoids these questions, it would be difficult to argue that postwar Stalinism and the planning process were substantially different than they had been during the 1930s. Nonetheless, the author offers the important insight that even under the most oppres-

sive political and economic Soviet system institutional components can exhibit some autonomy, so that the party autocracy might face surprising consequences.

Dunmore's book offers an original interpretation of the limits to absolute power in a command system. It also should be noted as a revealing study of the regional dimensions of an imperial political economy. His institutional and historical approach goes a long way to unravel the regional factors in this geographically and ethnically diversified country, an aspect meriting greater attention by Western sovietologists.

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JIRI VALENTA. *Soviet Intervention in Czechoslovakia, 1968: Anatomy of a Decision*. Reprint. Baltimore: Johns Hopkins University Press. 1981. Pp. xii, 208. Cloth \$12.95, paper \$5.95.

For reasons familiar to readers of this journal, many historians frown on attempts to write the history of events in the recent past. Yet there is a strong and legitimate public interest in recent history, and if professional historians will not meet that need, others will step in to do so. In particular, the political scientists, or at least those among them whose orientation continues to be in part historical, have manifested a lively concern with recent history, especially that of the Soviet Union and its socialist allies.

For the political scientist, however, the study of history is not an end in itself but a means for achieving a better understanding of the principles underlying changes in societies and governments, with the ultimate objective of being able to predict such changes. Special attention is devoted to nodal points in the historical process, among them the making of political decisions. An impressive literature has been created analyzing the factors entering into decision making, and theoretical models have been constructed to test the validity of the analyses. Soviet decision making has attracted particular attention, both because of its intrinsic importance and because of the secrecy with which it is surrounded. (The resulting paucity of documentary or other reliable evidence, it may be noted, is one of the principal reasons why many historians prefer not to work in this field.)

Jiri Valenta's book is a specimen of this kind of political science study; he defines it (p. x) as "an analytic and interpretive essay focusing on the Soviet decision-making process." As its raw material the book contains abundant historical data, but the historical content of the book is strictly confined within a political science framework that defines its

form, dictates its goal, and to a considerable degree restricts its value.

According to Valenta, one of the "historical conditions characteristic of the post-Stalin era" in Soviet politics is that there exists "a collective leadership within which no single leader possesses sufficient power or wisdom to decide (or willingness to accept responsibility for) all important policy issues" (p. 9). This assertion is supported by the dubious historical generalization that "the most crucial decisions of the post-Stalin era have been made by coalitions of senior Soviet leaders" (p. 18).

Old heresy becomes new orthodoxy and a new generation knoweth not the teachings of its fathers. Twenty years ago it was the established wisdom in political science circles that one-man rule is the norm in the Soviet political system and that collective leadership, when it appears, is merely a transitional stage characteristic of succession crises, which soon gives way to a new and more stable one-man dictatorship. Only the boldest ventured to challenge this view, which had behind it the weighty authority of Merle Fainsod's classic text, *How Russia Is Ruled* (1953).

Life is more complicated and unpredictable than theory, however, and the truth is that Soviet politics has reflected *both* one-man rule and collective leadership, in varying proportions, from its origins down to the present time. The exact situation at any given time must be ascertained by empiric observation, rather than by fitting the facts into an *a priori* scheme, no matter how fashionable it may currently be.

Khrushchev's overthrow, representing as it did a triumph for the principle of collective leadership, was naturally followed by an exaggerated stress on that aspect of the Soviet political system. For Brezhnev to accumulate power, under the new post-Khrushchev rules of the game, it was necessary for him to conciliate all the major influence groups in Soviet society, meanwhile cautiously strengthening his personal base by eliminating or weakening dangerous rivals. In effect Brezhnev has become the first Soviet leader whose power rests on his willingness to forego one of the basic attributes of leadership, the right to make decisions. The result is a failure at the heart of the decision-making machinery that leads to repeated blunders, among them the invasion of Czechoslovakia in 1968.

From the military standpoint, as Valenta shows, the invasion was "a smooth and remarkably effective operation" (p. 147). At the same time, however, it represented an "acute political failure" resulting from "political misjudgments" (*ibid.*). Valenta is fully aware of the defective quality of Brezhnev's leadership—his favorite adjective to characterize Brezhnev is "wavering"—yet somehow he never sees the connection between the two aspects of the situation. By

insisting on the collective leadership framework and by ignoring the Soviet system's real and continuing need for decisive, informed, and courageous leadership (qualities all too sadly absent from Brezhnev's performance), Valenta has missed the real meaning of the events he describes.

To end on a more positive note, Valenta's book is based on extensive research into relevant materials in English, Russian, and Czech and is enriched by a number of firsthand observations. As such it is a valuable contribution to the history of the Soviet invasion of Czechoslovakia.

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NEAR EAST

MAURICE LOMBARD. *Études d'économie médiévale*. Volume 3, *Les textiles dans le monde Musulman du VII^e au XII^e siècle*. (Civilizations et Sociétés, number 61.) Paris: École des Hautes Études en Sciences Sociales or Mouton; distributed by Walter de Gruyter, New York. 1978. Pp. 316.

This is the third posthumous work based on notes left by the late Maurice Lombard and published in a series entitled *Études d'économie médiévale*. The list of the original sources used in all three volumes appears at the head of the first volume only. At the end of the present volume there is a *bibliographie méthodique* to serve all three volumes. The study opens with an essay explaining how the vast Islamic domain reached into three areas of technologies that were then cut off from one another: the Byzantine (Egypt and Syria), the Oriental (Sassanid empire and India), and the Western (Spain). The large increase in the circulation of gold in the Arab world stimulated economic activities and led to "*le prodigieux essor urbain*" with great urban sprawls like Baghdad, Samarra, Cairo, Kairouan, and Cordova. The cultural unity and freedom of travel inside the Muslim world broke down the barriers that hostility and suspicion between the Byzantine and Sassanid empires had erected, restoring free flow of goods and technologies between East and West. Since his first celebrated article in the 1947 *Annales*, Lombard has presented the enriching and liberating function of Islamic culture as a counter thesis to Pirène's conception of the origin of the Middle Ages. The most important Islamic contribution to the development of the Mediterranean world in the Middle Ages was the textile industry. Of the four main textiles only wool and flax were produced in the Mediterranean zone. It was the Muslim empire with its open lines of communication that allowed the expansion of silk, cotton, and their technologies to the Mediterranean.

After this preamble, separate chapters discuss the

old textiles (wool and flax), the new ones (silk and cotton), the secondary (hemp, camel hair, vegetable fibers), and dyes and fixers. The final chapters describe the manufacturing centers of the Egyptian delta, volume and quality, and the productive forces. In the tradition of the *Annales*, maps and charts are essential: they must illustrate the production areas and the dynamic of the exchanges among zones where raw materials are produced, manufactured, and consumed.

The breadth and depth of Lombard's original research, especially in the Arab sources, is evident throughout. It is the best of the three volumes in which he intended to capture Islamic civilization in the hour of its greatest splendor, but its uneven quality is disconcerting. Few of the copious footnotes deal with materials published in the last thirty years; of course, Lombard's notes could not reach much beyond the late 1950s since he died in 1964. The massive bibliography shows a small portion of the titles published in the last fifteen years; the editors have, however, made no effort to update the volume with available, new data.

The most regrettable omission is the work of S. D. Goitein, in particular his *A Mediterranean Society I* (1967). This monumental synthesis of the Geniza papers could alone have provided vital data on prices, markets, shipping costs, weaving and dyeing processes, work conditions, and scores of other aspects of the textile industry. Lombard's volume thus contains several sections that should have been rewritten using findings available at the time of publication. Further examples are: on the introduction of the Merino sheep in Spain (p. 26), the information provided by R. S. Lopez ("The Origin of the Merino Sheep," *Jewish Social Studies* [1953]) should be considered; on the important question of fashion and variety in clothing, the information coming from the Geniza is ignored and so is the useful study by E. Ashtor, *Histoire des prix et des salaires dans l'Orient médiéval* (1969); on the existence of workers' corporations in Islamic countries, Lombard's position is convincingly challenged by M. S. Stern, "The Constitution of the Islamic City," and C. Cahen, "Y a-t-il des corporations professionnelles dans le monde musulman classique?" (both are in Hourani and Stern, *The Islamic City: A Colloquium* [1970] and there is additional evidence in Goitein, pp. 75–98); discussion of the silk exported from Sicily cannot ignore the data found in a document of the Byzantine diocese of Reggio edited by A. Guillou, *Le brébion de Région* (1974). One dating error occurs on page 202: Syracuse fell to the Arabs in 878, not in 861. The volume, nevertheless, is a valuable addition to the shelf of historians of the Middle Ages for the great contribution it makes and by providing the only comprehensive study of the textile industry of the medieval Islamic world whose

civilization Lombard perceptively and rightly defined as "*la civilisation du textile*."

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DANIEL PIPES. *Slave Soldiers and Islam: The Genesis of a Military System*. New Haven: Yale University Press. 1981. Pp. xxx, 246. \$25.00.

Recently, there has arisen an interest in the slave institution in Islam and its dominant role within so many Islamic armies; Daniel Pipes's book appears almost contemporaneously with Patricia Crone's *Slaves on Horses, the Evolution of the Islamic Polity* (1980). The latter, however, concentrates on the prehistory and tribal background of the slave institution and the interplay of political events in the first two centuries or so of Islam. Pipes does deal with this political and social background in his part 2, "Origins" (pp. 107–94), but those who are not professional Arab and Islamic historians but have an interest in comparative institutions and phenomena of divergence within societies with a common, if distant, Near Eastern origin, will find part 1, "The Islamicate context" (pp. 1–102), especially fascinating.

Pipes sees military slavery as an "Islamicate" feature (that is, a feature of Islamic civilization as a whole) yet as having no organic or necessary connection with the Islamic religious law or the ethos of the faith (one might nevertheless observe that Islam, with its emphasis on man's individual responsibility for working out his own salvation, made it perfectly possible for a slave to live a fully Muslim life, despite his inferior legal status). Other cultures have made use of slaves in war at odd times only: in pre-sixteenth-century Muscovy, in seventeenth-century Manchu China, and in the British West Indies toward the end of the eighteenth century, but the lesson of the American Civil War seems to show that domestic slaves cannot really be used for combatant purposes and retain their former docility.

The Islamic experience was different; military slaves could be recruited from peoples marginal to, or completely outside, the Islamic world—Turks above all, but also Circassians, Greeks, Armenians, Negroes, and so forth, put through a carefully organized training system to develop fighting skills, to instill loyalty to the ruler, and, above all, to bring about a transformation of identity, almost inevitably entailing conversion to Islam (as with the Janissaries, culled from the Christian peoples of the Balkans). Pipes's basic explanation for this is a hypothesis, yet a plausible and attractive one: disturbed by the disparity between the ideals of Islamic government and the brutal reality of tyranny and internecine strife, a disillusioned Muslim elite turned to exercising its leadership in nonmilitary fields, leaving the

military one open for slaves recruited from outside the Arab-Persian heartland. In this way, the civilian traditions of Islamic government were reinforced, and the concerned Muslim could wash his hands of much dirty reality (at the price, however, of eventual political sterility and decadence, later developments that lie outside Pipes's purview). The institution of military slavery is thus closely linked with the evolution of Islamic society; since a similar political evolution did not occur outside, neither did this military solution appear. Naturally, nuances will have to be made in Pipes's thesis; were, for instance, the free *ghāzīs* or fighters for the faith along the Islamic frontiers simpler souls with a bellicose piety who did not feel the gap between ideal and reality so keenly?

The second part of the book looks at the role of clients (*mawālī*) in earliest Islam as foreshadowing the enrolling of slaves from outside. Crucial here, Pipes holds, were the facts that the decline of tribalism and the momentum of conversion to Islam had by the early Abbasid period narrowed down the traditional fields of recruitment within the empire, but the only way to hold the loyalty and to instill some discipline into the new barbarian soldiers was to create a structured slave system.

As Pipes concedes, many of his ideas are speculative, but one feels that he is essentially on the right track. His book has an attractiveness of argumentation, grounded on the necessary solid research, that makes it a stimulating contribution to Islamic historical studies.

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KARL K. BARBIR. *Ottoman Rule in Damascus, 1708–1758*. (Princeton Studies on the Near East.) Princeton: Princeton University Press. 1980. Pp. xix, 216. \$20.00.

Thanks to the availability and use of Ottoman archival materials, the history of Syria and Palestine under Ottoman rule has become the focus of serious investigations during the last two decades. The Ottoman court records made available for scholarly use in both countries as well as research conducted in the central archives in Istanbul make us aware how the situation often described as anarchy had actually a complex social and political background. This situation needs a careful analysis, which might reveal to us certain developments with policies of readjustments on the part of the central government.

Based on Ottoman archival material Karl K. Barbir's work is a fresh effort in the same direction to make us understand the real historical process in the Ottoman province in a crucial transition period. The rise of local dynasties and growing resistance to the Ottoman central power in Syria were under-

standably viewed and emphasized by nationalistic local historians as positive developments. They tend, Barbir remarks, to reduce Ottoman Syria's history to a local history. He quite rightly shows a reaction to this trend and points out the risk of distorting reality by ignoring the Ottoman presence and policies. Syria's history developed, he stresses, within an Ottoman "framework." The Ottoman state was not "an inert and helpless observer" (p. 8). He nevertheless accepts the fact that the Ottoman state under the strains of a disastrous war against Russia lost control of provincial administration in the second half of the eighteenth century. In this period, a new framework with the powerful *ayans* or *hanedans* developed that ushered in a new era and a new adjustment by the central government. In this work Barbir discusses Ottoman policies for the pressing problems of controlling the rising local powers, *ayans* or "provincial patricians," the Janissaries, and the tribes in the first half of the century.

Throughout the book the importance of the organization and protection of the pilgrimage by the state is emphasized and seen as the determining factor for the peculiar policy of the Porte towards the authority and responsibilities of the governor of Damascus especially in his relationships with the *ayans*, Janissaries, and the tribes. For, "as commander of the pilgrimage, the governor of Damascus administered a vast economic social and military enterprise." Merchants of Damascus used the pilgrimage caravan to ship their goods to Mecca and imported goods from this area. This then must be the reason why the pilgrimage received the most detailed study in the book (actually, the last third of the book is devoted to it). Except for some attempts at re-interpretation of their roles, the governor's opponents in power struggles, the local military groups, the tribes, and especially the notables were not made the subject of the same kind of research in the book as the pilgrimage.

Some recent discussions of the *ayan* problem are totally ignored (in particular, see *Studies in Eighteenth Century Islamic History* [1977], edited by T. Naff and R. Owen [pp. 27–73]). Some details remain to be corrected. For example, fees for the *mubashir* (p. 26) were not entered as *income* but as *expenses* in the *tevzi defteris*. The local *ayan* were then to collect upon approval of this list of expenses by the central government the amount listed. Barbir's book as a whole is a very useful contribution in many respects to our understanding of the conditions in the Ottoman province of Damascus in the first half of the eighteenth century.

HALIL INALCIK
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ANN MOSELY LESCH. *Arab Politics in Palestine, 1917–1939: The Frustration of a Nationalist Movement*. (Mod-

ern Middle East Series, Middle East Institute, Columbia University, number 11.) Ithaca: Cornell University Press. 1979. Pp. 257. \$19.50.

Books written in English on the Arab-Israeli dispute are extremely numerous, but solidly researched and relatively dispassionate studies of the Palestinian Arabs are very few. Ann Mosely Lesch has contributed a concise, clear, useful, and balanced account of the Arab majority in Palestine during the crucial years of the mandate when their struggle for the right of national self-determination was crushed.

After reviewing the development of Zionism and the sympathy felt for it in Britain during World War I, Lesch analyzes the society, economy, and class structure of the Palestinian Arabs. While a Muslim professional middle class slowly emerged, leadership resided with town notables and large landowners who could do little more than protest hopelessly as the resident Jewish minority increased its relative advantage over the Arabs in education, organizational unity, and economic power. The constant feuding among the major Arab families and factions seriously weakened their ability to bargain with the British. Lesch argues, however, that general international support for paternalistic imperialism and British backing of the establishment of a Jewish national home in Palestine meant that little could have been gained from negotiations.

Increasing frustration, a sense of political injustice, and deprivation felt by the Arabs led to the emergence of new groups and parties and, ultimately, to the general strike and rebellion of 1936–39, which were suppressed by the British. The end result of Arab violence and World War II was a strengthening of the Jewish community's position and the discrediting and destruction of the Arab political leadership.

In addition to a chronological survey, Lesch has separate chapters on the generally low level of involvement of other Arab countries; the futile Arab diplomacy at Geneva and London, which made noncooperation one of the few tools left to the Arabs in Palestine; Arab responses to British proposals for a legislative council; and an analysis of the Arab use of violence. In her conclusion Lesch carries the story of the Palestinians to 1948 and assigns the failure of their national movement to internal disputes, British support of Zionism, and the irreconcilable aspirations of Jews and Arabs in Palestine that made compromise impossible.

Despite some repetition caused by the structure of the book, Lesch has succeeded admirably in her chief objectives of concisely telling the history of the Arab Palestinians, of putting their history into the broader context of British and international policy, and of placing the early Palestinian resistance movement in a framework allowing comparisons with

other such movements in the Middle East and elsewhere. This book does not displace the quite detailed two volumes on the same subject written by Yehoshua Porath and published in 1974 and 1977. Lesch has used the same wide range of sources but has written a more analytical study that presents the most important aspects of Palestinian Arab history with sympathy and insight.

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YAACOV BAR-SIMAN-TOV. *The Israeli-Egyptian War of Attrition, 1969–1970: A Case-Study of Limited Local War*. New York: Columbia University Press. 1980. Pp. xi, 248. \$19.50.

The Israeli-Egyptian "War of Attrition" of 1969–70 is the least known and perhaps the worst understood of all Israel's wars with its neighbors. Ezer Weizmann, former Israeli defense minister, has described it as "the first war in which Israel was defeated." Yet at the time this was not the perception of Israel or world public opinion, and even today this is denied by some. In a newspaper article on the tenth anniversary of the end of the war, General Haim Bar-Lev, who served as chief of staff in 1969–70, wrote of Egypt's "failure" in the war and contended that President Nasser, who initiated the hostilities, could not show any significant gains from the enterprise. Bar-Lev is, of course, like many generals before him, winning his Waterloo retrospectively. For the truth is, as Yaacov Bar-Siman-Tov demonstrates in his admirable study, that the "War of Attrition" was both a strategic and a political setback for Israel and as such marked a decisive turning point in the history of the Middle East conflict.

Bar-Siman-Tov's analysis is based on first-hand study of Israeli materials, interviews with Israeli decision makers, and published Egyptian data (of which he makes excellent, although necessarily less comprehensive, use). As a political scientist and strategic analyst he is concerned with placing the "War of Attrition" within a general theory of limited local wars and with exploring the interactions of military and political decisions and great power and local client relationships.

He divides the war into four main stages. In stage one (March to July 1969) Egypt imposed its attrition strategy. In stage two (July to December 1969) the Israeli air force moved in and broke down the Egyptian strategy of artillery-based attack. In stage three (January to April 1970) Israel imposed its strategy by air raids deep into Egypt. But (and this was decisive) Israel miscalculated the reaction of the USSR, which intervened directly by sending its airmen into action against Israel. In stage four (April to July 1970) there was direct confrontation

between Israel and the USSR, culminating in an air battle over the Gulf of Suez on July 30, 1970, in which Israel shot down five Russian-piloted planes.

The truce that ended the war was an apparent military stalemate, but, as the author argues convincingly, Israel's strategic position vis-à-vis Egypt had in reality deteriorated. Russian intervention had neutralized Israel's previous strategic superiority. The war ended without Israel finding an answer to the threat of the SAM-3 missiles to its air force, and Israel's dependence on the United States increased. Why did Israel, so soon after its decisive victory in the Six-Day War of 1967, suffer this major setback? Three reasons are adduced by Bar-Siman-Tov: first, reliance on the territorial status quo; second, underestimation of the enemy; third, reliance on defensive military doctrine. Yet, as Bar-Lev's blinkered hindsight shows, evaluation of the results of the war, even by those in a position to know better, was distorted by wishful thinking. The results, as the author points out, "were only too clear in the Yom Kippur War." The Egyptian troops who overran Bar-Lev's eponymous line on the Suez Canal in 1973 were building on that earlier achievement.

In its cool, step-by-step dissection of the war, this book will be of interest not only to Middle East specialists but also to strategic analysts, students of international relations theory, and contemporary historians.

BERNARD WASSERSTEIN
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SHAHROUGH AKHAVI. *Religion and Politics in Contemporary Iran: Clergy-State Relations in the Pahlavi Period*. Albany: State University of New York Press. 1980. Pp. xix, 255. Cloth \$39.00, paper \$12.95.

Written mostly before the victory of the Iranian revolution in February 1979, but updated to cover that revolution, Shahrough Akhavi's book is an outstanding piece of exposition and analysis of a difficult subject and has already been widely greeted by laudatory reviews. Even before the late 1970s it was clear to Akhavi, as to a few other scholars who rarely followed up their insight, that the Iranian clergy or *ulema* still held an extraordinary political and often oppositional position that made their history a highly worthwhile subject of study. Possibly because others had studied in some depth the political role of the *ulema* in the late nineteenth and early twentieth centuries, when they were important in a successful protest against a tobacco concession to a British subject, and then in the constitutional revolution of 1905–11, Akhavi chose to concentrate on the Pahlavi period, particularly on the rule of Mohammed Reza Pahlavi, 1941–79. Under his father, Reza Shah, Akhavi shows how bureaucratic

centralization; dress and education codes for *ulema*, teachers, and lawyers; and changes in mores and Westernization, including for women, greatly reduced the power and income of the *ulema*. There was a considerable revival of the *ulema* in the years when Mohammad Reza was a constitutional ruler, 1941–53, but in the 1940s and 1950s relatively few religious figures were involved in oppositional politics—notably the anti-British maverick Ayatollah Kashani, who for a time was allied with Mosaddeq and earlier with the Fedayan-e Islam, a fundamentalist terrorist group whose assassinations of leading figures succeeded in terrorizing many into taking more antiforeign positions. The ideology of the Fedayan often sounds like that of Khomeini. This was, however, more because of the similarity of fundamentalist ideologies than because of any ties, as Khomeini in the 1950s refrained from politics and wished to enter them only when he had high *ulema* allies, whereas the Fedayan, as Akhavi notes, were relatively lower class and neither well educated nor members of the *ulema* strata.

After Mohammad Reza's 1953 restoration by coup, the *ulema* extorted from him as a price of thorough support an anti-Baha'i campaign, including the destruction or confiscation of Baha'i buildings. The Baha'is, in 1981 the object of even more severe clerical attack, spring from a splinter religion from Islam and hence are not regarded as a legitimate religion like the Christians and Jews. The chance location of the Baha'i center in Israel and their religion have helped make the whole huge minority (three hundred thousand to five hundred thousand—the largest minority religion in Iran) seem suspect to many Iranians, and the position of Baha'is is precarious under any clerically influenced government. Akhavi's detailed account of the anti-Baha'i campaign of the mid-fifties provides useful background for more recent and more violent events.

Akhavi deals with such matters as changing organization of religious endowments (*waqfs*), showing how under Mohammad Reza Shah the new government-appointed *waqf* administrators skimmed off large quantities of land to give to royal favorites, such as the now turncoat General Fardust. He also discusses the rise of the city of Qom as the center of Iranian Shi'ism under the Pahlavis, replacing the Shi'i holy cities in Iraq, and notes the technical reforms introduced by the single top Shi'i leader of the 1950s, Ayatollah Borujerdi. Under Borujerdi the *ulema* were enjoined to stay out of politics, which is one reason why Khomeini, who had already written an anti-Pahlavi treatise in the 1940s, became politically quiet until after Borujerdi's death in 1961. Borujerdi's one really political venture was his effective opposition to a land reform law of 1960. In 1962, under U.S. and other pressure, the shah and

his ministers launched a more effective land reform, and Akhavi indicates which *ulema* were opposed to it—neither opposition nor favor was voiced by Khomeini. In the same year a local elections law giving votes to women and implying that non-Muslims might vote aroused more universal opposition, including that of Khomeini, who became the leader of antigovernment activity in 1963 and was exiled to Iraq in 1964.

Khomeini's writings and activities from Iraq, Paris, and finally Iran are well covered, as are those by religiously oriented secularists—Bazargan, Bani Sadr, and especially Ali Shariati. Going beyond Akhavi's time span, we can see that the *ulema* have, for now, won back the courts, schools, property, and power, and have added to these unprecedented control of the government. Akhavi's fine book indicates how this could happen.

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AFRICA

STEPHEN BAIER. *An Economic History of Central Niger*. (Oxford Studies in African Affairs.) New York: Clarendon Press of Oxford University Press. 1980. Pp. 325. \$45.00.

The appearance of Stephen Baier's *An Economic History of Central Niger* has been heralded by the author's publication of several learned and impressive articles on the economic history of central Niger. This book follows the high standard of scholarship and excellence that Baier demonstrated in his earlier articles, and the result is a splendid study that makes a major contribution to the study of African economic history.

This work deals with the economic history of the West African Sahel from 1850 to 1960 and focuses on the area of central Niger, particularly on Damagaram, a precolonial state, and Damergu, an area north of Damagaram. The book examines the interplay of the local economy and outside forces during the period under study and presents balanced information about the desert and the savanna areas. Baier analyzes pastoral and sedentary production and examines the mechanism of interaction between the two sectors in terms of regional trade and climatic cycles.

Between 1750 and 1900 the economy of the savanna area of the region experienced rapid economic growth, and by 1900 an extensive trading system that included agriculture, animal husbandry, and craft production linked the area with the trading system of the region.

Until about 1900 the two primary influences in central Niger were the trans-Saharan trade and the

expanding economy of the desert's edge. The growth of the trans-Saharan trade brought opportunities to merchants of Damagaram and subordinated them to forces beyond their control, especially dependence on commercial capital. Much of this capital came from Europe and entered North Africa through Tripoli. The trans-Saharan trade declined after 1900 because of several factors that included unsafe routes, competition of the coastal trade, decreasing transportation from the coast to the area, and British and French taxation of the trans-Saharan trade. Furthermore, transportation on competing routes became cheaper after 1900.

With the imposition of colonial rule at the end of the nineteenth century, the economy of central Niger had to adapt to the colonial economy. Along with French colonial rule came arbitrary taxation in kind, direct taxation, forced labor, and military recruitment. The period of the imposition of colonial rule also coincided with climatic changes. Especially devastating for the region was the drought of 1911–14, which came after a long period of relatively good weather.

The period 1900–20 was a time of economic decline both in the desert and in the area of the Sahel and the savanna under French rule. The economy retained a strong involvement in the regional trade in the staples of West African markets, however, as the livestock trade illustrates.

Baier uses the concept of the open economy, which is a model of relations between the center and the periphery, to describe the economy of central Niger between 1930 and 1960. During this period the economy of central Niger was dependent on the metropolitan French economy, and the economic policy of the French administration as well as private activities like banking worked to ensure the flow of primary products to France in exchange for imported merchandise.

Baier's book, by examining the impact of the trans-Saharan trade on the Sahel, human adaptation to climatic change, and the impact of colonial rule, makes a major scholarly contribution that will be welcomed by African economic historians as well as policy planners for central Niger.

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GRAHAM CONNAH. *Three Thousand Years in Africa: Man and His Environment in the Lake Chad Region of Nigeria*. (New Studies in Archaeology.) New York: Cambridge University Press. 1981. Pp. xx, 268. \$59.50.

It was Edmund Clerihew Bentley, humorist and satirist (1875–1956) who remarked: "The art of Biography/Is different from Geography./Geog-

raphy is about maps./But Biography is about chaps." Graham Connah's offering, presented by Cambridge University Press as an item in their series *New Studies in Archaeology*, has a title that might suggest a consolidated account of the past three thousand years in the Lake Chad region. Any such assumption is doomed to disappointment. The area of Connah's choice is part of the ancient historic kingdom of Kanem-Bornu, divided now among a number of emirates. The author claims "to examine both the archaeological and the historical evidence . . . in the context of Man/environment relationships," drawing heavily "on ethnographic observation both in the present and in the recent past."

Connah's work opens with the section "Ways of Looking at the Past," which a senior student immediately dismissed in my presence as "an undergraduate essay." The author makes a plea "for total history," as if there were something new in it and as if it had not been said often by Africanist historians since the early 1950s. He then discusses a number of "models" in an analysis of the African savanna and Lake Chad. This would have been interesting enough in some ways were one not aware that in this fundamentally Islamic region there are documentary sources as well as sources in oral tradition; there may also be many privately owned manuscripts, of which an earlier generation of historians was unaware, such as those found in a number of parts of Africa, including Nigeria.

There follows a discussion of environment and man in northeastern Nigeria, based on a list of ninety-six sites, all of them apparently unhistorical. The remainder of the work discusses the artifacts disclosed by excavations, apparently chiefly *sondages*, at several sites: I was reminded of a remark made in my presence by Sir Mortimer Wheeler about the late Professor Garstang—that he had dug more holes in the Middle East than any other man. Two final chapters, on urbanization and state development, and a summary of a number of aspects of human adaptation in the area avoid easily accessible documented history in the area together with the surviving private papers of Sir Henry Richmond Palmer, located in various archives.

By all means let historians realize that archaeology and its ancillary studies are a necessary ingredient in the understanding of African history. But Connah needs to realize that historians are concerned to establish a detailed and accurate historical sequence from every source available to them, especially in relation to individual men who have risen above their fellows as leaders and rulers. Archaeology, perhaps, is no longer solely concerned with artifacts: it is concerned with every aspect of ecology. But this must not be allowed to obscure man's political and religious aspect. It is perhaps the severest criticism of Connah's work that, in an Islamic area, the index

discloses not a single reference to Islam, to a mosque, to Islamic culture and education, or even to a Muslim divine.

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England

TIMOTHY C. WEISKEL. *French Colonial Rule and the Baule Peoples: Resistance and Collaboration, 1889–1911*. (Oxford Studies in African Affairs.) New York: Clarendon Press of Oxford University Press. 1980. Pp. xix, 323. \$49.50.

Late in the nineteenth and early in the twentieth centuries, the Baule peoples of the central Ivory Coast experienced their first direct and prolonged contact with Europeans. During those years, periods of prosperity and calm intermingled with others of great violence, culminating in a brutal repression of Baule resistance. This account of the French conquest and the responses that it provoked is a clear, thorough, and meticulously documented piece of scholarship. Enormously valuable as a microhistory illustrating the mechanics of the colonial process at the local level, its analysis of the motivations and reasoning of the parties involved also makes a major contribution to the field.

Timothy C. Weiskel introduces the Baule with an essay on their precolonial social, political, and economic structures, which, he asserts, many French officials did not understand. In the detailed account of the relationship between the Baule and the French that follows, it becomes clear that their misperceptions often led the colonizers to adopt policies whose consequences they could not fully comprehend. The book concludes with an examination of the nature of the Baule response and places the events described in a broader historical context. This is an especially useful endeavor, for it is relevant to researchers in all areas of colonial history, not just West Africa.

The book might be said to concern itself with a series of rival viewpoints whose inherent friction helped produce the tragedy of the Baule experience. Weiskel uses the phrase "working misunderstanding" to describe the contacts between the French and the Baule, but he also discusses the impact on the Baule of the conflicting desires of French parliamentarians to limit colonial spending, and officials on the spot to meet immediate problems; the sometimes divergent interests of local administrators and governors; and the recurrent competition between civilians and soldiers for colonial power. Such circumstances rendered a consistent policy all but impossible, and French vacillations led the Baule to conclude, until the campaign that sealed their fate, that France would ultimately withdraw from their land. In explaining why France

finally moved to crush the Baule, the author makes an important point, convincingly demonstrating that it was not the unchecked ardor of the military that precipitated the decision (as had happened in the Sudan). Instead, an economic concern (the need for the Baule to join the colony's labor force) prompted a civilian governor to demand an armed expedition. Similarly, it had been the unsettling effects of the French presence on Baule labor patterns that had stimulated Baule resistance in the first place.

Although this book is occasionally repetitive, the spelling of names of African localities and individuals is not consistent, and there are lengthy French passages that may deter nonspecialists, the book's strengths far outweigh its weaknesses. Weiskel is straightforward when he cannot fully answer a question, and his comments point the way to useful new research. Finally, the maps provide excellent terms of reference, and the photographs are superb—several of them speak more hauntingly of the dehumanizing aspects of colonialism than could any number of words.

KENNETH J. PERKINS
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CHRISTOPHER M. ANDREW and A. S. KANYA-FORSTNER.
The Climax of French Imperial Expansion, 1914–1924.
Stanford: Stanford University Press. 1981. Pp. 302.
\$29.50.

Between the causes and effects of modern imperialism lies the complicated history of political maneuvering through which colonial expansion was accepted by the home government. No study has presented any aspect of that history with greater clarity or critical analysis than *The Climax of French Imperial Expansion*. Put quite directly, this is a remarkable book, one that demands to be read, and one that will be assured a prominent position in the literature on the subject.

Long concerned with the role of that unique French pressure group, the *parti colonial*, Christopher M. Andrew and A. S. Kanya-Forstner here trace its multiple activities and several successes during the travails of the World War. It was in the face of the ignorance of a premier like Viviani or against the long-established opposition of a premier like Clemenceau that members of the *parti colonial* managed to expand French interests in the Near East and West Africa. This was something of a virtuoso performance, played in part on the fear and distrust that the French had of the British in colonial matters and on the complementary French concern with national prestige. As the authors aver, "the unanswerable argument of national prestige" strengthened the colonial party's "otherwise unappealing expansionist demands" (p. 70).

Given the rather symbiotic relationship between French imperialism and British, the narrative closely follows the diplomatic negotiations between both countries and the attitudes of the men who formulated policy in each nation. What one finds is suspicion and contempt on both sides, a contrast with the appearance of unity in the war effort. The "old time" diplomacy of imperialism continued to prevail even in this time of troubles. But the authors demonstrate that the French were not so well prepared for a resolution of that activity as were the British. When peace came, sudden and unexpected at the time, the French had not yet drawn up their peace aims. The government's position on Syria, the centerpiece of French expansionist ambitions, was not ready until six weeks after the peace conference had begun.

Yet even in victory and the fullness of an extended empire, the French were not deeply attracted by imperialism. For a brief moment after the war there was exultation, but this quickly died. "The *parti colonial* could not make public opinion colonialist. But at moments of crisis it could none the less enlist public support for overseas expansion by presenting colonial issues as questions of national prestige" (pp. 29–30). And so it did during World War I.

The Climax of French Imperial Expansion upholds this thesis well, doing so through a close review of government documents and personal memoirs and by a fine comparison of the thinking and plans of the major participants on both sides of the English Channel. Moreover, the narrative is most readable, presented in a style that includes a touch of irony and a pleasing number of felicitous quotations.

RAYMOND F. BETTS
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THOMAS Q. REEFE. *The Rainbow and the Kings: A History of the Luba Empire to 1891*. Berkeley and Los Angeles: University of California Press. 1981. Pp. xx, 286. \$24.95.

The Luba empire, a dynastic state of the Sudanic type, one of the largest in Central Africa in the eighteenth and nineteenth centuries, had its heartland roughly between the parallels of 7° and 8° south latitude and was bounded on the west and east, respectively, by the Lomami and Lualaba rivers. An expanding frontier had carried the fringes of this empire many kilometers from the center, and allegiance was claimed from tributary states over a vast expanse of savanna country, from the southern fringe of the Congo rain forest down to what is now northern Zambia. What remains of the heartland today comprises a population of a mere fifty-seven thousand persons.

The origins of the Luba empire are shrouded in a hazy past, but it appears to have predated the

Bakongo kingdom of northern Angola that was in existence by A.D. 1400 and of which it was perhaps the progenitor. Throughout the long centuries of its rise the Luba empire, lying at the center of the continent, appears to have been insulated from the outside world. The salt and iron resources of the heartland provided the basis of the empire's wealth and prestige, which were such that distant states sought to claim common ancestry with it. The formation and expansion of this empire, the complexity of royal succession disputes, special relations between the Luba royal dynasty and client states, lineage power brokering, intrigue and ritual murder, resource manipulation and military conquests, and a Luba genesis myth (a forest of overlapping symbols transmitted through proverbs and free oral narrative) form the subject matter of Thomas Q. Reece's book. It is necessary to wade through this material to get a "synchronic view of Luba political ideology . . . highlighting important institutions and principles."

The story is brought up to 1891, by which year the empire had ceased to dictate the course of events in this part of Central Africa. By the 1870s and 1880s it was in disarray under the impact of ivory and slave traders from the west and east and their exploitation of succession struggles. The arrival of the Congo Free State forces hastened the final destruction, and by the 1890s the empire was no more than a palsied thing. Branches of the royal dynasty, however, survive today, providing chief administrative officers and four ruling heads (kings) of surviving branches of the Luba dynasty. The Luba were a nonliterate people, and Reece relies much on oral tradition for the earlier part of his study. It takes on increasing interest, however, when he draws on accounts of European missionaries, explorers, traders, and colonial-era documents of the nineteenth century: they bring concreteness and life to the tale.

The author appears to have read all extant material pertaining to his subject and is impeccable in his methodological approach. He is at his best when describing his sources and methods of research: he spent eleven months in Zaire and was affiliated as research associate to the National University of Zaire; he had the support of Zaire government officials in his research; field work was largely composed of interviews, through an interpreter, with informants ranging in age from forty-five to eighty-five years; four Luba "kings" were also consulted. The results of this field work are collected in the form of twelve tapes and 807 pages of notes, now deposited with Indiana University. There is solid completeness in the well-displayed list of contents, maps, tables, and figures in this book; reference notes are full and detailed but awkwardly placed at the end of the text. The index is adequate. The main weakness of this study is that African

personalities rarely emerge as more than faceless abstractions. This is a book primarily for the specialist.

R. W. BEACHEY
University of Waterloo

FREDERICK COOPER. *From Slaves to Squatters: Plantation Labor and Agriculture in Zanzibar and Coastal Kenya, 1890–1925*. New Haven: Yale University Press. 1980. Pp. xv, 328. \$25.00.

In this sequel to his first book, *Plantation Slavery on the East Coast of Africa* (1977), Frederick Cooper follows the story through the abolition of slavery and the establishment of colonial rule. It is foremost a regional study of the penetration of capitalism by the colonial state seeking to convert communal land into freehold plantations producing export crops and African cultivators into a landless labor force. We have seen this story before in the growing literature on Kenya; what is distinctive about Cooper's analysis is the detail with which he charts the responses of the different groups involved: indigenous farmers who avoided involvement in the capitalist economy except as temporary day laborers to preserve their own production, former slaves and immigrants who sought economic security by squatting on the plantations while resisting labor demands that interfered with their own cultivation, migrant workers who invested much in their journey and worked full time to amass enough money to return home, and Arab landlords who sought to maintain the paternal relations of the slave plantations while ensuring an adequate labor supply by encouraging the squatters to settle on their lands. The result was a stalemate for economic development, with the landlords controlling the means of production but unable to attract sufficient labor to exploit them and the squatters having the labor but lacking the security of tenure to develop the production of cash crops.

Cooper tackles a number of the thorny issues of African economic history as well. He shows how race and ethnicity can be flexible characteristics responsive to changing relations of production, how African cultivators made rational economic responses both in avoiding the fate of landless laborers and in retaining or in many cases reverting to subsistence production in response to colonial policies, and how structural and causal analyses can be combined to produce a fully detailed historical account.

There is also a broad comparative sweep to Cooper's work. Sustained comparisons are drawn not only to postabolition development in the New World but also to the rise of capitalism in the Old. The two processes are linked, and Cooper is able to show how the prevailing ideology of capitalism

supported both abolition and the peculiar brand of capitalism imposed on the colonies while demonstrating how the colonial situation produced different results.

Cooper's first book was noteworthy for its combination of meticulous scholarship and insightful analysis. This one is even better, the analysis deeper and more mature, but the broad range still brings its problems. Comparative materials can still be intrusive and poorly integrated, analytic points are frequently repeated two and even three times, and the different geographic areas are covered unevenly: the materials and resulting analysis for the coast of Kenya, where Cooper was able to do field work, are superior to those for either Zanzibar or hinterland Kenya. The book remains, however, an excellent one, as important to students of slavery, development, and labor history as it is to students of Africa.

THOMAS SPEAR
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BONAR A. GOW. *Madagascar and the Protestant Impact: The Work of the British Missions, 1818-95*. (Dalhousie African Studies Series.) New York: Africana. 1979. Pp. xvii, 266. \$42.00.

To one with some knowledge of the British missionary thrust into Africa during the nineteenth century, the Madagascar experience related by Bonar A. Gow seems almost an idealized model. All the tensions created when missionaries from Europe arrived in a non-Western environment were there: acceptance, rejection, competition, and exclusivity. Gow has selected his title and subtitle with care, for they precisely describe the limits of his study. He deals with two primary themes: the manner and effect of Protestant missionary activity and the Malagasy, primarily Merima, response to the missionary presence and to the ideas of Christianity. His research was primarily carried out in the archival materials of the four English-speaking Protestant missionary societies with stations in Madagascar. He has little concern with the activity of the Norwegian (Lutheran) Missionary Society or with the work done by the French Roman Catholic missionaries; nor is he prepared to evaluate Malagasy history beyond the responses to Protestant Christianity. Gow's product, therefore, has to be viewed as an aspect of nineteenth-century British imperial history.

The book contains six substantive chapters. The first three trace chronologically the phases of missionary activity and the relationships between missionaries and various Malagasy peoples. There is a familiar pattern. The missions were initially accepted by the Merima as a source of education and technological advancement; expulsion followed as a new monarch realized how alien ideas undermined

the traditional order and religion. With yet another succession, missionaries were invited back as a counter to French advances on another part of the island. The chronology concludes with an assessment of the relationship between the Protestant missionaries and the Merima in a chapter in which Gow's anger with missionary attitudes of superiority becomes a substantial subtheme.

The last three chapters are topical in construction. One is a study of the education and medical work of the missionaries; another focuses on the development of an independent Malagasy church and the English response to it. The final chapter is a fascinating analysis of the political adaptation of Christianity by the Merima government. The queen and her government on the advice of her prime minister embraced Christianity and established a palace church, free from links to the European missionary community. It was a Protestant success, but one that the Europeans viewed with grave misgivings because of their exclusion from the center of political power and because of the direct linkage of church and state, which to the Congregationalists was extremely distasteful. This chapter on the climax of Protestant activity on Madagascar wonderfully illustrates tensions, attitudes, and compromises inherent in the missionary movement of the nineteenth century.

Besides the six substantive chapters there is a preface reviewing the literature, an epilogue surveying the effect of French rule on the Protestant missions, a summary essay, a bibliography, and an index. *Madagascar and the Protestant Impact* makes a valuable contribution to the knowledge of European cultural expansion and the manner in which those ideas were received and altered—in this case by the Merima of Madagascar.

JAMES B. WOLF
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LEROY VAIL and LANDEG WHITE. *Capitalism and Colonialism in Mozambique: A Study of Quelimane District*. Minneapolis: University of Minnesota Press. 1980. Pp. xii, 419.

In 1806 a Portuguese governor reported home that agricultural progress in the Portuguese estates of the Zambesi was being held back by a class of ignorant and lazy *senhores*, who lived off African production without contributing anything to it. The legacy of this class still had not been lived down when Portuguese rule in Mozambique ended in 1974, and Leroy Vail and Landeg White have explored with clarity and sensitivity the contradictory forces that impinged on this region of Mozambique. The reactionary tendency was rooted in the

prazo and the concessionary company, which relied less on their efficacy at organizing production than on preventing anyone else from doing so. The dynamic tendency came from small traders, who sought to connect peasant producers with overseas markets, and from capitalists, mainly British, who attempted to use various forms of coerced labor to turn lackadaisical *prazos* into productive, if brutal, plantations. The Portuguese state, meanwhile, vacillated and trembled before foreign pressure but slowly moved from a liberal policy of encouraging more thorough exploitation in the mid-nineteenth century to a rigidly nationalist conception under Salazar of making Mozambique into an economic appendage of Portugal. Either way, however, Portuguese policy foundered on its dependence on the very class whose limitations the state was trying to overcome.

Vail and White follow the intersection of these conflicting tendencies in the Quelimane district of Mozambique over more than a century and a half, from a period dominated by the slave trade to a modest expansion of peasant production to the carving up in the 1880s of territory into concessions that built on the *prazo* structure, collected taxes, monopolized local trade, and turned their regions into "private labour pools" (p. 147). Out of this system came a minor sugar boom. When Salazar abolished forced labor and the *prazos* in the 1930s, government taxes already equaled six months' wages, and compulsory cultivation of cotton and then rice was added to the people's burden; the sugar company made sure that the new tasks were done by women, leaving male labor for the cane fields. Direct Portuguese administration turned out to be a worse reign of terror than that of the companies. By the time the Portuguese regime fell and the companies proved unable to survive without it, the peasantry of the Quelimane district expressed not so much hope as relief: "FRELIMO is letting us rest" (p. 391).

This is an excellent book, part of a slowly expanding literature on colonial Africa that combines a sense of the specificity of the encounter between African peoples and European capital and states with an awareness of the most basic issues and the total economic and political context. No other historical work on Africa has got as far inside a European plantation as Vail and White have done in the case of the Sena Sugar Company, using company records and interviews with workers to follow changing patterns of management and labor control over generations. The authors have not only examined revolts, mass flight, and individual and collective efforts to find the least awful alternative in a system designed to present no alternatives at all, but they have also used a rich tradition of songs from the region to re-create peasants' and workers' per-

ceptions of the changing social structure of oppression.

The most striking virtue of this book is also a minor flaw. The great "isms" of the title sit solidly on the cover; the complexity of their meaning and relationship to each other is illuminated by the narrative but not reflected on by the authors. Nevertheless, Vail and White have taken the discussion from an elevated plane, where abstract concepts determine structures, into the plantation itself, where capital and state were caught up in the limitations of the very social relations of production they sought to transform and where workers, however much the planters tried, could not be reduced to nothing more than labor power.

FREDERICK COOPER
Harvard University

GIDEON SHIMONI. *Jews and Zionism: The South African Experience, 1910–1967*. New York: Oxford University Press. 1980. Pp. xiii, 428. \$27.50.

On the international scene today there are two major pariahs: the state of Israel and the Republic of South Africa. Each has been ostracized and stridently criticized by a majority of United Nations members, and the two have often been paired as racist powers, two peas in the same imperialist pod.

Relations between Israel and South Africa and the role of South Africa's more than one-hundred-thousand-member Jewish community in shaping those relations have been the subjects of much popular and propagandistic writing, especially in the past decade. Gideon Shimoni, a South African by birth and for twenty years an Israeli by choice, has provided us with the most thorough, scholarly, and even-handed treatment to date. The author has consulted and cited an impressive array of archival materials in English, Hebrew, and Afrikaans.

Shimoni skillfully traces the history of South Africa's Jews, who are predominantly of Lithuanian origin and who have long been among the most ardent Zionists in the world. Although the Jews in South Africa have prospered and in general enjoy a comfortable, indeed luxurious existence, there have been periods when they have felt the sting of antisemitism. For example, in the depression-ridden 1930s virulent antisemitism promoted by South African fascists found fertile soil, particularly in the anti-British Afrikaner community. Legislation restricting immigration was enacted first in the form of the Quota Act in 1930 and again in 1937 to reduce the Jewish element in the South African population.

Despite the foregoing, South African Jews have been a privileged minority by reason of their whiteness, and they exercise basic rights that are denied

to black, colored (mixed race), and Indian groups because of race. Periodically Jews are reminded that their acceptance in South Africa is dependent on their unconditional acceptance of Afrikanerdom and its white supremacist policies.

They are held answerable for the foreign policies of Israel vis-à-vis African states, especially the Republic of South Africa. For much of the 1950s and 1960s Israel sought allies in the newly emergent black African nations as an integral part of its global strategy aimed at cultivating Third World support and thus averting diplomatic isolation. Consistent with this strategy, from time to time Israel went beyond most Western powers in castigating the apartheid regime in Pretoria. A few incidents are illustrative. In 1961 the Israeli prime minister, David Ben-Gurion, and the president of Upper Volta issued a joint communiqué criticizing racialism in South Africa and Portuguese policies in Angola. The same year Israel voted at the United Nations in favor of censuring Eric Louw, South Africa's foreign minister and a long-time Jew hater, for making "offensive, fictitious and erroneous statements." The fundamental dilemma of South African Jewry was pointed up when both the Zionist Federation and the Jewish Board of Deputies repudiated the Israeli position. Still, South Africa's Prime Minister Verwoerd chided South Africa's Jews for Israel's unfriendly behavior at the United Nations and rebuked the Jews for supporting the opposition Progressive party. Questions of South Africa's Jewish loyalty were raised again in 1962 when Israel voted for sanctions against the republic.

All of these developments are vividly chronicled and well documented by Shimoni, who sees the Six-Day War of June 1967 as a watershed in the relations between South Africa and Israel. One could argue that the formal break in Israeli-black African diplomatic links and the concomitant rapprochement between South Africa and Israel were really later phenomena. Certainly in 1971 Israel still believed it was possible to remain on good terms with African states, for it was in that year that it offered to contribute 10,000 Israeli pounds to the Liberation Committee of the Organization of African Unity. Once again a brouhaha ensued in South Africa, and Jews there were put on the defensive. Of course, since 1973 when Prime Minister Vorster, who had been interned during World War II for pro-Nazi activities, visited Israel, relations between Pretoria and the Jewish state have been "normalized." Unfortunately, Shimoni's excellent book does not deal with these latest developments, but scholars and laymen alike can learn much from his examination of the period to 1967. For those concerned with the seething cauldrons of the Middle East and South Africa's white *laager*, the Shimoni book is a must.

ROBERT G. WEISBORD
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ASIA AND THE EAST

NORIKO KAMACHI. *Reform in China: Huang Tsun-hsien and the Japanese Model*. (Harvard East Asian Monographs, number 95.) Cambridge: Council on East Asian Studies, Harvard University; distributed by Harvard University Press, Cambridge. 1981. Pp. xvi, 384. \$15.00.

This book provides us with the best treatment in English of the career of Huang Tsun-hsien (1848–1905), an important late Ch'ing diplomat, bureaucrat, and political thinker. Noriko Kamachi describes the entirety of Huang's career, from junior diplomat in Japan in the late 1870s to provincial official in the exciting days of the 1898 reforms. Her use of sources is wide ranging and is particularly to be commended for extensive incorporation of Japanese scholarship. The result is a solid, comprehensive, and balanced, if not an excitingly written, work.

Enough of Huang's personal life is incorporated into the narrative to enhance the discussion of his career and his ideas. The reader appreciates knowing the significance of Huang's South China Hakka background and how close he was to the women in his life, from great-grandmother down to his wife. Kamachi also stresses the importance of Huang's poetry and translates a good deal of it. He was highly regarded as a poet, and he frequently used poetry to express political and even diplomatic opinions as well as to reveal his emotional sensitivity.

Kamachi claims that Huang was very ambitious and, having had only indifferent success in the traditional examinations, turned to a career pattern of diplomatic service and patronage by high officials, always hoping to enter the central arena of power in Peking. In fact, he wanted to be the "Cavour of China." His views on the need for China to enact reforms and to summon up a renewed vigor in its body politic evolved over the decades. These views were strongly shaped by his five years in Japan (1877–82) and even more so by his three depressing years in the U.S. (1882–85) at a time of virulent Chinese exclusion fever. A romantic, he developed a great admiration for the Japanese Meiji government's reforms as being rooted in the heroic achievements of a few courageous men. In the late 1890s, Huang's major treatise on Japan helped to infuse many young Chinese reformers with a romantic view of themselves as heroic "founders," comparable to the Meiji founders.

Kamachi gives us a capable analysis of Huang's views on Japan, which were basically formed during his five years' residence there and then incorporated into his large treatise on contemporary Japan in the late 1880s. Consistent with the conservative tenor of the times, however, Peking officialdom took no interest in this carefully crafted and perceptive piece of scholarship until after the Sino-Japanese War of

1894–95. Then Huang came into his own and was closely identified with many of the post-1895 reformers. In fact he might have risen to become one of the empire's top officials had not his career been cut short by the political coup that ended the 1898 reform movement.

In sum, this is a worthwhile book that fits nicely among existing works on late Ch'ing reformers from Wang T'ao to Chang Chih-tung, Yen Fu, and Liang Ch'i-ch'ao.

DANIEL H. BAYS
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MARY BROWN BULLOCK. *An American Transplant: The Rockefeller Foundation and Peking Union Medical College*. (Center for Chinese Studies, University of California, Berkeley.) Berkeley and Los Angeles: University of California Press. 1980. Pp. xxvii, 280. \$17.50.

Mary Brown Bullock's study of Peking Union Medical College, like recent works in Chinese economics and other disciplines, presents evidence of numerous continuities between the pre-1949 and post-1949 periods. She joins other scholars in arguing that the cumulative skills and strategies developed during the 1920s and 1930s contributed significantly to rapid growth under the People's Republic of China. In the first instance, there was carry-over in personnel; by following the careers of PUMC graduates, Bullock is able to demonstrate that they have held a high percentage of leadership positions in the Chinese Medical Association and in the China Medical College under the Chinese Academy of Medical Sciences. Research on health problems especially prevalent in China—parasitic diseases, for example—has built on studies conducted by PUMC. Even some of the strategies used by the People's Republic for the delivery of medical care were being tested by PUMC in the 1930s: the use of paramedics and midwives, hierarchical organization of medical facilities to coincide with the political structure, and so on. The dilemma of whether to concentrate limited resources on training specialists or "barefoot" doctors plagued PUMC administrators as well as People's Republic leaders. The preference of scholars and professionals for the urban centers is a well-known and continuing phenomenon, although Peking now has the power to require some work and experience in rural China.

Despite these illustrations of the contributions of PUMC to medical research and education in China, Bullock expresses considerable ambivalence about this flagship of the Rockefeller Foundation in Asia. Ambivalence is, of course, too mild a term for the attitude of many Chinese toward the social service activities and educational institutions sponsored by Western philanthropists and missionaries in China.

Why does she use this apologetic tone that shades over into hostility?

As Bullock indicates, the aid of the Rockefeller Foundation amounted to a kind of benevolent despotism. Despite Sinification of administration and curriculum, the power of the pocketbook gave the Rockefeller Foundation in general and the Rockefeller family in particular veto power over major policy and personnel decisions. Conflict between the religious and moral goals and the demands of scientific education was never fully resolved, although it gradually became less serious. Also contributing to misunderstanding were the overblown ambitions of Westerners out to reform the world under the banner of Christ. Reform on a national level required nationwide power and organization, something not attained by Westerner or Chinese until the Communist party came to power. To evolve programs appropriate to the Chinese environment required time: time for experimentation, time for training cadres, time for adapting Western techniques, and time for testing out traditional Chinese ones. In this latter realm, PUMC contributed not nearly as much as the philanthropists had hoped, but more than has generally been recognized until Bullock's book appeared.

Bullock's method of presentation has weaknesses as well as strengths. Using a wide variety of Western sources, she spells out the ambitions of the Rockefeller Foundation, the gradual scaling down of goals, and the ensuing conflicts. Then she turns to the contributions of PUMC and her ambivalence is replaced by appreciation. Much of the history is told through the lives of a few influential leaders, which makes for interesting reading, but which also makes the integration of themes more difficult.

JESSIE G. LUTZ
Rutgers University

JOHN WHITNEY HALL *et al.*, editors. *Japan before Tokugawa: Political Consolidation and Economic Growth, 1500 to 1650*. Princeton: Princeton University Press. 1981. Pp. xiv, 392. \$22.50.

Japan before Tokugawa focuses on institutional developments in Japan in the so-called *Sengoku*, or Warring States period, stretched in this book to cover the century and a half between 1500 to 1650 when Japan emerged out of feudal chaos into a consolidated, politically integrated unit under the hegemony of the Tokugawa house. Another in a growing series of conference volumes on premodern Japan, *Japan before Tokugawa* is linked to the others by the commitment to multiple essays (dealing mainly with institutional history) and the organization and intellectual leadership of John Whitney Hall of Yale. The volume's introduction is attributed to three editors but is the usual fine job of synthesis we have come to expect from Hall.

The book is a welcome addition to the sparse coverage of this period in Western languages. Indeed, the composition of this volume is a measure of the underdeveloped state of the field. Of the eleven essays, nine are by Japanese scholars and only two by Americans—Hall and Kōzō Yamamura. At the conference where these papers were first presented, the Japanese scholars worked closely with American collaborators, including such well-known scholars as Marius Jansen and George Ellison, to produce a readable English version of the original Japanese.

The results are somewhat mixed, as is always the case with collections of essays. Some authors deal with details of local economic or political development, providing necessary nitty-gritty information not available in English, such as Matsuoka Hisato's piece on Ōuchi family rule in western Japan. Others write at a higher level of abstraction—Asao Naohiro's thoughtful piece on "Shogun and Tennō," for example. Thematically, at least four essays focus broadly on economic matters—commercial and urban policies of various daimyo, imposition of systems of local economic control, or overall growth during the period. Three essays deal with the two major political figures of the day, Oda Nobunaga and Toyotomi Hideyoshi. Three more deal with the nature of daimyo political and administrative control, and one essay crosses several areas in discussing the development of cities during this period of growth and change in Japanese history.

Reactions to these essays will differ depending upon readers' interests; certainly no brief review can attempt to evaluate them all. Suffice it to say that this reviewer was most impressed with the broadly conceived, more theoretically oriented essays: Asao's "Shogun and Tennō," Sasaki Junnosuke's "The Changing Rationale of Daimyo Control in the Emergence of the Bakuhun State," and Fujiki Hisashi's "The Political Posture of Oda Nobunaga." There should be something here for everyone—unless of course your interests are religion, art, literature, drama, or anything cultural. The conscious decision to focus upon institutional developments may leave some readers unhappy, but the editors give more than ample justification for their decision. This is an important book that fills some gaps in our knowledge of a neglected era of Japanese history, and perhaps it will stimulate further inquiry by Western scholars.

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ANN BOS RADWAN. *The Dutch in Western India, 1601–1632: A Study of Mutual Accommodation*. Columbia, Mo.: South Asia Books. 1978. Pp. x, 159. \$15.00.

The Dutch played a minor role in India's colonial history. Their main effort was reserved for Indone-

sia, the source of most of those spices that were the magnet attracting all Europeans to seaborne Asia in the sixteenth and seventeenth centuries. For the Dutch, India essentially was the place from which they could get trade goods, especially cotton cloth, to use to pay for the spices. Ann Bos Radwan's agreeable little book fills in a gap in this larger picture. It tells us as much as we are ever likely to know about the early years of the Dutch in one of the major Indian exporting areas, the province of Gujarat, and especially its main port of Surat. The book is based on the fragmentary Dutch records at the Hague.

There are several themes running through the book. One is the problems the Dutch faced in buying cargoes for their ships in Surat. European goods found little favor in the Indian market, so the Dutch (and also the English) were forced either to pay in bullion or to raise money locally by means of expensive short-term loans. A second theme is relations between the three European countries trading in Gujarat early in the seventeenth century: the Dutch, English, and Portuguese. The years under discussion saw the eclipse of the Portuguese and the Dutch ascendancy over the English. The third focus of the book is relations between Dutch and Indian merchants. The latter had important advantages: local knowledge, adequate capital, and business acumen. Thus, it is not surprising that they usually did better than the Dutch and that in most matters the Dutch had to accommodate to local commercial practice. Another theme is the changing policies of the directors of the Dutch East India Company, although these changes had little effect on practice in Gujarat. Slow communications and the fatuity of some of the directives from the top meant that Dutch merchants on the spot were forced to make ad hoc decisions on their own authority. Finally, we are given detail on the relations of the Dutch with the rulers of Gujarat and with the Mughal emperors and their officials. Here matters went less smoothly. The basic problem was that the Dutch wanted a legally binding treaty to enable them to trade in India, but such a document was unknown in Mughal diplomatic practice.

The main value of this book is that it makes available to the nonreader of Dutch the contents of the Dutch records for this place and period. There are problems: several themes are not worked out as cogently as could be desired; Radwan's transliterations of Indian words are sometimes a bit astray; there are many misprints and eccentric use of semicolons; a line left out on page 64; and footnotes out of sequence in chapter 2. Nevertheless, this book does quite adequately fill a gap in our knowledge of the early activities of the Europeans in India.

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PAUL HOCKINGS. *Ancient Hindu Refugees: Badaga Social History, 1550–1975*. (Studies in Anthropology, number 6.) New York: Mouton Publishers; distributed by Walter de Gruyter, New York. 1980. Pp. xiv, 285. DM 75.

Ancient Hindu Refugees: Badaga Social History, 1550–1975 by Paul Hockings is a local history. It concerns a people, known as “Northerners” or Badagas, who migrated southward from the Mysore plateau (now in the state of Karnataka) into that projection of the Western Ghats called the Nilgiris (or “Blue Mountains”). These people moved to escape the increasing insecurity and turbulence that came in the wake of the disintegrating Vijayanagara empire. They settled among the indigenous Todas, Kurumbas, and Kotas and gradually developed into a separate community. Numbering only a few hundred to begin with, hardly more than 500 in 1603, and scarcely 2,200 in 1812, when first brought into an Indian (imperial) census, they increased rapidly. So much was this so that today there are about 120,000 Badagas. Living in some 370 villages, they are mainly occupied in profitable commercial farming. Their success has largely been due to their coming into a relatively congenial environment and to their own remarkable adaptability. They came into a temperate climate where natural resources (for food, water, and so forth) were plentiful; into a land where there was plenty of space to settle and to farm; and into a social setting of tribal peoples with whom the Badagas did not compete and who, therefore, did not find the Badagas threatening. To this must be added—by the very fact of their having already been uprooted and having already had to adapt to change—an openness of mind and a willingness to learn new ways. By emulating British planters, they acquired new agricultural techniques, adopted new commercial crops, and took advantage of newly available loans. Through the influence of missionaries, they gained an appreciation for modern education at all levels. Such appreciations, however, did not come without costs. There were profound inner struggles and social torments to the extent that around 1860 some saw Badaga society coming to an end. But by about 1905, after missionaries had become less abrasive and when Christian Badagas were no longer excluded from the community, such turmoils calmed down. At the same time as commercial farming became more and more profitable, Badagas became prosperous. Ootacamund, the former hill station and now thriving city, became their cultural and social center. A considerable minority now no longer depends upon agriculture, but has moved into the professions, creating a new urban class of Badaga doctors, lawyers, teachers, engineers, technical experts, and civil servants.

The story of Badaga success as a process of adaptation to a new environment and to a complex of changing conditions and circumstances is the central theme of this study. It is a theme treated in much detail and with thoroughness, critical detachment, methodological precision, caution, modesty, and a lively imagination. Although the author is an anthropologist, his treatment of the subject may perhaps be more pleasing to historians than to other social scientists. His study is singularly free of the jargon and the theoretical shibboleths of contemporary ethnographic literature. His bibliography, rich in the breadth and depth of source materials that were consulted, also seems to show some disdain for the work of the last decade. His long and close acquaintance with the people who were (and are) his subject and his reliance upon personal observation seem no less obvious. The result is a work in which emphasis upon empirical data is heavy and emphasis upon abstract speculation—variously garbed in forms of currently fashionable “scholasticism”—correspondingly light. This is refreshing. Those looking for some intellectual catchword or ideological slogan, therefore, may be disappointed. Those looking for a sound contribution to their understanding of local history in South India, however, will not be disappointed. Like the great district manuals of old, this is a work of originality that will withstand the tests of time.

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STEPHEN FREDERIC DALE. *Islamic Society on the South Asian Frontier: The Māppīlas of Malabar, 1498–1922*. New York: Clarendon Press of Oxford University Press. 1980. Pp. ix, 290. \$49.00.

Stephen Frederic Dale's insightful study of the Māppīlas of Kerala, which covers an unbroken time span of more than four hundred years (1498–1922), constitutes a major breakthrough in the study of the social and cultural history of South Asian Muslim communities. The occasional monographs that we do find on the Muslim regional communities are basically ethnographically oriented, history being treated as a backdrop. Examples of this are: S. C. Misra's *Muslim Communities in Gujrat* (1964) and Ghaus Ansari's *Muslim Caste in Uttar Pradesh* (1960).

The Māppīlas, an important coastal community of Muslim traders with agrarian roots in the interior of South Malabar District, remained until recently without a political power base. As Dale notes, on the internal frontier the Māppīlas confronted the politically dominant Hindu majority, on whose land they had to eke out their livelihood; on the external frontier they confronted the European powers who destroyed the Māppīlas' lucrative overseas trade

monopoly. The Māppīlas were able to cope with these external and internal frontiers by asserting their corporate identity as militant Muslims. In fact, their prolonged armed conflicts with the Europeans and the Hindus resulted in the creation of a "Muslim heroic ideal." In order to seek redress from social and economic grievances, the Māppīlas, aided by the *ulema*, legitimized violence against their adversaries as *jihad* (holy war). The military odds in such confrontations were heavily weighted against the Māppīlas. Hence, in Kerala, as among the Muslims of Southeast Asia, "the *jihad*, normally the tactics of an attacking or expanding state, became a suicidal act, an expression of desperation and defeat." The *mujahidun* (holy warriors) actively sought the glory of becoming *shahids* (Islamic martyrs). The Māppīlas, in fact, continue to this day to revivify the ideal of *shahids* in their annual *nerccas* festivals. According to Dale, in the long run such methods became a viable means of defending the interests of the rural Muslim population: their internal frontier in South Malabar actually expanded at the expense of the Hindus, helped, no doubt, by the Māppīlas' active conversions of lower caste Hindus.

The agrarian disputes in the nineteenth century, when South Malabar came under British jurisdiction, were major Māppīla outbreaks that were directed mostly against the *janmis* (the landed Hindu gentry), who were terrorized and frequently murdered. There is evidence to indicate the crucial and dominant role of the two Māppīla *ulema*, Sayyid 'Alawi and his son Sayyid Fadl. They were respected Mambram *Ta'ni'als* (leaders of the Māppīlas at Mambram in South Malabar) and sanctioned acts of violence against the *janmis*. With their blessing, Māppīlas who attacked the Hindus became *shahids*. The evidence that emerged out of the British reports of the late nineteenth-century administrators, such as Logan, indicted the faulty British land tenure settlement in Malabar as the true cause of the outrages. But the British did nothing to change the land tenure system that they had newly established and that had adversely affected the position of the land-poor Māppīlas. The resentments of the Māppīlas, therefore, remained intact.

The All India Khilafat movement of 1921 took a drastic turn in Kerala. The Māppīla rebellion (1921–22), which the Muslim *ulema* led with the full support of the rural Māppīla population of South Malabar, was "an end product of Muslim frustration and resentment towards the British and their policies, what they considered to be subversion of the Islamic community." The Māppīlas construed that Islam was in danger and simply wanted an Islamic state of their own that would right the wrongs perpetrated on them. Dale correctly observes that the rebellion was not a great peasant revolt as has been interpreted by some scholars. As he aptly puts it, "the Muslims defined their goals in religious

rather than in economic or in class terms." Dale's analysis of the rebellion, however, is largely based on Tottenham's and Hitchcock's books on rebellion, augmented by the Muslim and official British versions. The Malayali journals, magazines, and newspapers of the early 1920s, in which Hindus and Christians articulated their perceptions of the rebellion, are left out of the analysis.

The central thesis that Dale tries to argue in his book is the tenacious nature of this Islamic community in South Malabar, confronting hostile frontiers over the centuries. The survival and expansion of the Māppīlas was due to their unflinching faith in militant Islamic ideology, vitalized by their memories of the *shahids*. It is a fitting climax, as Dale points out, that the Māppīlas should get a Muslim majority district created for them in 1969 by the Kerala state. Dale has convincingly proven his case by means of his dexterous marshaling of the vast data and the excellent manner in which he has presented it. In arguing for the Māppīlas' quest for Islamic identity, however, he may have unwittingly pinned them down as unmitigated adversaries of the Hindus and Christians. Despite the Māppīlas' self-awareness as Muslims, they speak Malayali, dress like Nayers, and, in their kinship and other social customs, they have imbibed the local norms. Dale could have discussed some of the accommodative and adaptive strategies followed by the Māppīlas over the centuries in order to live in comparative peace and harmony with their non-Muslim neighbors.

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MARTIN J. MURRAY. *The Development of Capitalism in Colonial Indochina, 1870–1940*. Berkeley and Los Angeles: University of California Press. 1980. Pp. xii, 685. \$34.50.

This certainly is a well-documented book. The bibliography (50 pages) apparently lists everything available on the subject—from published works to unpublished archives and Ph.D. or M.A. theses. The 500 pages of text (organized into nine chapters, an introduction, and a postscript) are supported by exactly 1,731 footnotes that add up to no less than 130 pages of smaller print. Notwithstanding all this, the author, Martin J. Murray, fails to deliver the last word on the topic. Technically, too, there are drawbacks. The chapters do not follow one another according to any internal logic; the style is generally too didactic; and the reasoning is quite ponderous. In fact, there are several unnecessary repetitions in the book, due, I assume, to the author's insistence on being understood.

The story of the introduction of capitalism to Indochina is the familiar one of any colonial country annexed, ruled, civilized, and developed by a Western capitalist power. It consists of the imposition of

the capitalist system as it existed in the mother country at the time of conquest onto a colony that may not have been in any way prepared to receive it. The author argues quite well that the introduction of capitalism into Indochina was "not the consequence of the self-propelled logic of local conditions. Instead, the capitalist mode of production was imposed from the outside through administrative force and violence" (p. 474). The resulting transformation of Indochinese society was highly traumatic as capitalism and colonialism colluded to turn a great part of the Indochinese people into a working class of wage laborers who had had no preparation or transition into the changed circumstances. The characteristics, processes, and evolution of all these economic and social changes are well described, fully analyzed, and profusely documented with statistics in this book. Murray goes into some detail to demonstrate the fallacy that "colonial Indochina represented a unified political entity, consisting of a large, generally homogeneous, "peasant" subsistence sector, upon which had been artificially grafted a small, but dynamic, European-dominated capitalist sector" (p. 224). He also relates with great care how the twin rule of capitalism and colonialism affected the autonomy of the Indochinese villages. Among other things, the traditional authority of the notables was shattered, and in the rural communities of Indochina there was created "a class of property owners in effective possession of concentrated means of production and a class of property-less direct producers forced by economic necessity to sell their laboring capacity" (p. 482). The author devotes numerous pages to the working class and to the "agrarian social structures and rural class relations," but he says nothing at all about the bourgeoisie and the indigenous capitalist class that did, after all, come to play a crucial role in the development of capitalism. Nor can we find any mention of the newly created urban centers. This is all the more surprising as Murray adopts an avowedly Marxist-Leninist method of analysis that, although it chooses to emphasize the unequal classlike relation between the mother country and the colony, disregards local class conflicts. Along with the limitations mentioned earlier, this omission leads me to conclude that we must hear more on this particular topic. Incidentally, the French expressions, titles, and phrases used in this book too frequently exhibit errors of grammar or orthography.

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FRANK FARRELL. *International Socialism and Australian Labour: The Left in Australia, 1919-1939*. Sydney: Hale and Iremonger. 1981. Pp. xx, 284. Cloth \$24.95, paper \$11.95.

Making a detailed study of socialism, leftist movements, and labor parties in Australia is a courageous undertaking. The reason is simple: it takes great patience and unusual perspicacity to unravel the incredibly complex relationships between men, movements, parties, and politics in Australia, because the left has been bedeviled by splits, rivalries, personality conflicts, lack of support, defections, and subversion by opposition governments. These misfortunes happened mostly because socialism had different meanings for its different adherents, ranging from dogmatic communism in theory and practice to Fabianism in theory and almost reformist capitalism in practice. Such compromises on ideology were necessary to attract the swing vote, which decided election victories in Australia. They were all the more important as radical extremism is not a characteristic of the vast majority of the Australian electorate, even of the segment voting for the Labor party. Leftist radicals could therefore be found more often in the unions than in the Labor party. A further complicating factor for the left has been the existence of specifically Australian realities, which are quite incompatible with socialism and pose unusual challenges to politicians and parties seeking political office. They had to reconcile, for instance, the White Australia policy, isolationism, Commonwealth membership, and military conscription with socialist anti-racism, internationalism, anti-imperialism, and pacifism.

Frank Farrell traces in his very detailed study the efforts that were made by the various leftist parties, groups, and factions to cope with these complications. He also examines how or why they succeeded or failed. If the reader, especially one unfamiliar with Australian politics in general, finds the story somewhat bewildering, it is owing to the facts of the situation rather than to any failure on the author's part. Farrell has done a commendable job describing the organizations, programs, policies, and practices of the Australian left and the vicissitudes of its victories and defeats. The account, in addition to having inherent interest for an understanding of the political history of Australia between the two world wars, can also serve as an excellent introduction for an understanding of the Australian Labor party and other leftist groups today. It can explain, in particular, why the Labor party, which still considers itself the guardian of socialism, shows so few traces of the doctrine in its political practice.

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JEROME M. CLUBB et al. *Partisan Realignment: Voters, Parties, and Government in American History*. (Sage

Library of Social Research, number 108.) Beverly Hills: Sage Publications. 1980. Pp. 311. Cloth \$18.00, paper \$8.95.

A generation ago, V. O. Key, Jr., theorized that "critical elections" periodically recast the partisan loyalties of American voters, alter the balance of party power, and initiate policy change. Demoting Key's theory to a "realignment perspective," Jerome M. Clubb and his co-authors stretch critical elections into four realigning eras (1828–40, 1860–74, 1896–1910, 1932–46) of sustained policy innovation. Although a period of decaying partisanship and policy inertia followed each era, a new realignment (except for our own time) eventually revived effective government.

More than any previous work, *Partisan Realignment* emphasizes the linkages between voter realignment, partisan control of government, and policy outputs. Although its authors carefully hedge their conclusions and acknowledge limitations of data, their account of political history outruns the evidence they muster.

About half the book examines indicators of electoral realignment and decay. "Historical realignments," the authors note, "appear primarily as surges for or against the parties" without a "massive reshuffling of voters" (p. 114). This finding becomes explicable when we are later told that party loyalists never change their minds; newly eligible voters, nonpartisans, and politically apathetic citizens alone respond to the changing circumstances that produce realignments. The authors study voter groupings, however, without resort to the multivariate methods capable of unraveling party coalitions. They rely first on aggregate-level correlations of voting returns that are more sensitive to distortions of aggregation than regression measures and second on a decomposition of changes in moving averages of party percentages that cannot indicate the relationships between particular elections and select an arbitrary number of elections for comparison.

Electoral data also fail to disclose the specified eras of realignment and decay. A "realigning surge" emerges for the Democratic presidential vote in 1840, 1848, 1868, 1876, 1920, 1932, 1960 and for the Republican tally in 1876, 1896, 1920, 1932, 1952, and 1964. Six of the ten listed elections actually occurred during periods of decay. Indicators of decay are likewise inconclusive. For example, voter turnout was low during the 1920s (except for 1928) and a third party appeared in 1924 (and immediately disappeared), but the GOP controlled the national government throughout the decade. No party dominated government during the 1880s, but turnout was high and support for the parties remarkably stable.

Since "patterns of electoral change alone are not sufficient for the demarcation of . . . partisan realignments" (p. 155), the authors turn to "governmental and policy consequences of electoral change" (p. 157). The book's most interesting and original chapters explore the partisan control of national and state governments, uncovering such trends as a long-term decline in congressional partisanship. Still, the desired periodization fails to emerge, and the authors fall back on linking realigning eras with "policy initiative, innovation, and coordination" (p. 244) and decay periods with a paralysis of government.

Policy change now becomes both the central result of electoral realignment and the final arbiter of whether such realignment has occurred. Yet the author's interpretation of American governance is supported only by an assurance that "most of us would agree" with it "without demanding . . . detailed evidence" (p. 252). How then did Reconstruction come to unravel during the Civil War realignment? Is the realignment perspective consistent with McKinley era complacency or FDR's retreat from policy innovation; with the activism of James K. Polk, Benjamin Harrison, Woodrow Wilson, and Harry S. Truman; or with recent civil rights, welfare, medical, and regulatory legislation?

Partisan Realignment usefully challenges mechanistic interpretations of realignment and sensibly reunites politics with political change. But its realignment perspective seems sufficiently flexible to accommodate whatever results investigation might reveal. No future surprise is likely to daunt scholars committed to the viewpoint of this book.

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WALTER NUGENT. *Structures of American Social History*. (Samuel Paley Lectures in American Civilization.) Bloomington: Indiana University Press. 1981. Pp. xiii, 206. \$12.95.

In this little book Walter Nugent approaches American demographic history from the standpoint of the social historian. He begins by discussing the perspectives on the subject found in modernization theory and in the writings of Frederick Jackson Turner and Thomas L. Malthus. He was inspired also, he tells us, by the Annalists, notably Fernand Braudel, whose framework of "event," "conjuncture," and "*longue durée*" he has appropriated to his own purposes in dividing our population history into three eras characterized by different rates of growth. Of these, one of *longue durée* extended after a "*statistical Vorzeit*," from about 1720 until the early 1860s, during which the American population in-

creased by approximately 35 percent each decade. There followed a "conjuncture" between the 1860s and 1910 when this rate of increase dropped to some 24 percent. Thereafter, a second period of *longue durée* perhaps began in which mean increase per decade stands around 13 percent at the present time but may ultimately drop to much less.

In describing the birth and death rates and the immigration flows that produced these results, Nugent elaborates social typologies characteristic of the demographic eras: the first period of *longue durée* was dominated by the frontier-rural mode of life; the "Great Conjuncture" was a time in which that living style shared the stage with the rising metropolitan way of life; and our latest *longue durée*, if so it proves, is dominated by the latter style. He also distinguishes three other patterns of living, frontier-urban, settled-rural, and slave society, that he suggests were "misfits." These were modes of living that were out of step with the prevailing social patterns. As he describes the various major elements of his scheme, Nugent discusses a broad range of social implications and consequences.

In the early part of this book Nugent develops parallels between America's demographic history and the movements in Beethoven's Pastoral Symphony, displaying an enthusiasm for his subject that somewhat surpasses Braudel's description of demographic growth as "that vegetable expansion." But—such exuberance aside—given the present state of historical research in American demographic development, this small volume is an extremely useful survey of what we know and, by implication at least, of what we do not know about the subject. Even so talented a synthesizer as Nugent sometimes works violence upon the distinctions of primary researchers and jumps, in matters of controversy, in directions contrary to the preferences of readers. But he is very frank in admitting that he has not drawn out all of the implications of his subject matter. The footnotes are abundant and meaty, and the bibliography is a highly useful one, despite some peculiar omissions. This book deserves to be popular among both those seeking a general introduction to the demographic foundations of social history and among historians and graduate students in search of research topics.

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WILLIAM A. HAVILAND and MARJORY W. POWER. *The Original Vermonters: Native Inhabitants, Past and Present*. Hanover, N.H.: University Press of New England, for the University of Vermont. 1981. Pp. xx, 326.

This study by two anthropologists trained in archaeology presents evidence pertaining to Vermont Indians from Paleoindian times to the present. Although the explicit aim is synthesis, not detail, historians will find plenty of the latter. Indeed, the book has been carefully researched and contains a good bibliography. Evidence is often illuminated by anthropological wisdom, so that readers will learn a good deal about anthropology in general, especially archaeology, and Vermont Indians in particular.

The introductory chapter provides background data on linguistic affiliations, habitat, chronological periods, dating techniques, and archaeological and ethnohistorical methods. At the time of European contact in the seventeenth century, the main occupants of Vermont were the Algonquian-speaking Western Abenaki plus some Mahicans in the southwest corner of the state. Materials in the remaining chapters are presented in chronological order.

Chapters 2 to 4 deal with the traditionally defined prehistoric periods: Paleoindian, Archaic, and Woodland. Sites are identified and evidence is discussed and interpreted within the contexts of environmental conditions, comparable archaeological materials from beyond state borders, and with respect to time provenience. Ethnographic analogies are employed to make sense of the material remains. For example, in line with present knowledge about hunter-gatherers, we learn that Paleoindians of some 11,000 years ago were not simple wanderers eking out a bare existence. Rather their technological know-how allowed them to exploit seasonally a rich habitat consisting of several species of herbivores, seals from the Champlain Sea, and other foods. Following the retreat of the glaciers, Indians may have temporarily vacated Vermont since the earliest Archaic remains reflecting an adaptation to a forest habitat post date the latest Paleoindian sites by about 2,000 years. The Woodland period follows the Archaic and is distinguished by the appearance of pottery and the bow and arrow. Horticulture was not adopted until about the fifteenth century A.D., and then only when it did not disrupt prior seasonal subsistence activities. The Woodland period was terminated by European intervention in the seventeenth century.

Although the archaeological evidence is carefully evaluated, it needs to be stressed that much controversy still prevails over certain issues. For example, the authors suggest that the Vergennes Archaic culture, a regional manifestation with hybrid traits characteristic of the surrounding area dating from about 3,500 B.C. to 2,000 B.C. involved Algonquian speakers who were the lineal ancestors of the Abenaki. Not all agree. The complexity of the debates is better understood by referring to the bibliographic notes section where conflicting views and specific sources are given. Since these notes are

not keyed to the text as this reviewer believes should have been the case, it is easy for the nonexpert to accept the authors' persuasive interpretations without due consideration of contrary evidence.

Chapter 5 is an ethnographic portrait of Western Abenaki culture on the eve of European intervention. Given the scanty early historic evidence, certain areas are fleshed out with comparative Eastern Abenaki data. Unfortunately, since much of this information was collected by field ethnologists long after contact, parts of the portrait may reflect the affects of change. One may question the aboriginal origin of family-band hunting territories characteristic of the Penobscot, bifurcate collateral (or Hawaiian in the bibliographic notes) kinship nomenclature, and the absence of totemic clans (Gordon Day's evidence to the contrary), in the light of such historic events as population decline from disease, endemic warfare lasting for over a century, and rather intensive missionizing activities by Jesuits, all of which began in the early seventeenth century. In fact, chapter 6 presents an excellent discussion of how French, British, and Dutch influences altered traditional subsistence, social, and political organization.

The last chapter, although brief, discusses some of the important issues confronting modern-day Abenaki. Despite some 370 years of acculturation, the Abenakis have not lost their identity as a distinct people, one that they are trying to preserve, in part, by gaining governmental recognition of certain rights to which they believe they are entitled.

This well-bound book is enhanced by sixty-five illustrations, six tables, four appendixes (all pertaining to modern-day Indians), and the previously mentioned bibliographic notes. Despite some qualifications, the book is a good one and should prove useful to historians and anthropologists alike who are interested in Indians of the Northeast. The synthesis of the scattered and often highly technical archaeological sources is especially valuable. Those interested in reading further about the historical Western Abenaki should refer to the publications of Peter Thomas and Gordon Day.

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MARY MAPLES DUNN *et al.*, editors. *The Papers of William Penn*. Volume 1, 1644–1679. Philadelphia: University of Pennsylvania Press. 1981. Pp. xv, 703. \$28.50.

This first volume of William Penn's papers contains a selection of the extant Penn manuscripts through 1679. The major portion of these documents deals with Penn's conversion to Quakerism and his many efforts on behalf of Friends. In this period of his life

Penn changed from the fashionable son of a wealthy father charged with securing his father's Irish possessions to the wealthy and respected representative of his sect intent on improving his shaky finances. In his mature years he continued to defend his faith from attacks by outsiders and apostates, dabbled in a colonial venture in New Jersey, and sought to further Whig fortunes in England. These efforts are familiar enough to historians, but for the first time a substantial body of well-edited Penn manuscripts for these years appears in print.

Compared to other collections of papers published within the past three decades, this volume is relatively successful. Mary Maples Dunn and Richard S. Dunn, have been more selective in their choice of manuscripts to print than were the editors of the Jefferson and the Franklin papers a generation or so ago. The decision is a happy one as Penn's editors thereby avoided redundancy. The editing of the manuscripts is more literal than in many other modern collections, basically a wise decision for a seventeenth-century figure like Penn because the papers as published here retain the original flavor and meaning, although the decision to print superscripts does not seem to add anything for the reader and quite likely increased publication costs.

Unlike many other major collections, the editors decided not to include Penn's pamphlets with the edited contemporary manuscripts, and one wonders if all significant pamphlets will receive a well-edited modern edition (there is promise that the religious pamphlets will). In the future volumes the editors could include political pamphlets instead of such documents as Penn's account books that have been included in the first volume and are available to scholars on microfilm.

The notes are full and very helpful, although a few minor problems exist—as notes follow documents, subjects needing identification in several instances were introduced several pages before the document itself. There are also cross-reference problems with the notes: references are to documents, not the page or pages in the published volume, an annoyance when one has to rummage through either the table of contents or the index or both after encountering a reference to a point raised elsewhere in the volume.

These minor problems aside, this is a handsomely printed, well-edited volume that amply justifies continued efforts to print the papers of prominent people.

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AUBREY C. LAND. *Colonial Maryland: A History*. (A History of the American Colonies.) Millwood, N.Y.: KTO Press. 1981. Pp. xviii, 367. \$25.00.

Colonial historians have often neglected Maryland. For years this interesting proprietary colony has even lacked a comprehensive, integrated survey of its history. This omission is ironic, since the early graduate seminars at Johns Hopkins devoted extraordinary attention to Maryland and produced many fine monographs. Professional attention declined for years, however, and Maryland did not receive a comparable level of attention until a recent generation of graduate students began to exploit the rich archival resources of the Hall of Records. Between these two periods of greater research activity, Aubrey Land worked steadily as probably the most devoted scholar exploring Maryland's colonial past. Appropriately, Land has now written the long overdue survey history of early Maryland as part of the bicentennial series, "A History of the American Colonies." Building on four decades of personal research, he integrates here his own findings with the best of the older institutional and economic studies and the first fruits of the recent scholarly revival—generally material published by 1977—in a smoothly written narrative. With *The Chesapeake in the Seventeenth Century* (1979), this volume should restore Maryland to a more prominent place in the attention of colonial historians.

Land postulates three persisting themes in Maryland history. First, he stresses the continuous peopling of the colony for 150 years and the important diversity of those immigrants. Second, he traces the evolution of a provincial way in religion, economy, politics, and society, as Marylanders forged new practices sharply divergent from the ways of the old world. Third, he chronicles the struggle for autonomy, originating in battles with the proprietors and ultimately concluding in independence and statehood. Throughout, Land intentionally emphasizes the distinctiveness of Maryland, but he also instructively notes the many similarities with Virginia and other ways in which Maryland more closely resembled the Middle Colonies.

The provincial politics of the colony provide the narrative framework for Land, who weaves analysis of social and economic developments around a continuing account of the relations among proprietors, governors, and legislatures. The author argues that the Calverts succeeded more than any other contemporary proprietors. He follows closely the policies and evaluates the abilities of the five Barons Baltimore who presided over Maryland and assigns failing marks to only the last of the line, Frederick. Land becomes particularly at home in untangling the complicated politics of the war years and the colorful rivalries of the Dulaneys and Carrolls, who receive much attention in this study. Land employs broader strokes and less detail for his earlier canvas of the 1600s. Indeed, fully two-thirds of the book focuses on the final sixty years before the Revolution.

An especially welcome feature is Land's rich description of the economic differences that distinguished Marylanders. He effectively illuminates the lives of poor farmers in crude huts as well as the affluent elite in their stylish Georgian homes. With apt examples, Land traces the entrepreneurial activities that brought great wealth to some planters and merchants, while also sympathetically relating the economic conditions and other misfortunes that trapped a larger number of colonists in a life of poverty or bare sufficiency.

Consistent with the intent of this series, Land carefully balances detail and generalization, without footnotes, in attempting to appeal both to historians and a broader audience. He largely eschews controversial interpretations. Readers will readily subscribe to most of his conclusions, perhaps questioning most the disproportionate emphasis on the years after 1715 and regretting the occasional intrusion of minor factual errors. Everyone should concur, however, that this readable volume admirably addresses a serious void in the literature. Land's book should quickly assume a respected place in this series.

DAVID W. JORDAN
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WILLIAM E. NELSON. *Dispute and Conflict Resolution in Plymouth County, Massachusetts, 1725–1825*. (Studies in Legal History.) Chapel Hill: University of North Carolina Press. 1981. Pp. xi, 212. \$19.50.

This little book sets out to examine "the techniques by which disputes were resolved" (p. 4) in Plymouth County, Massachusetts, during the period 1725–1825. After an extensive examination of church and court records, liberally spiced with tabular presentations, statistical formulas, and "a 95-percent level of confidence," William E. Nelson concludes: "The intrusion of outside forces and ideas into otherwise insular Plymouth communities led to the appearance of groups that were unable to have their disputes adjudicated by their adversaries in the towns in which they resided" (p. 151); that is to say, over time, parties took their differences to court, rather than to the congregation.

Thus the book's first weakness is its failure to tell us anything new. Litigation multiplied in post-Revolutionary Plymouth County for the same reasons that it multiplied in post-Revolutionary Massachusetts and, for that matter, in the post-Revolutionary United States: more people and more trade. Litigiousness varies directly with density of population and volume of commerce, foreign and domestic.

Instead of laboriously preparing tables purveying such speculative data as "Estimated Number of Noncommercial Intracommunity Cases before and after Rise in Litigation" (p. 119), Nelson would have served his inquiry better by approaching it in a qualitative rather than quantitative manner.

That way, we could have learned in much more detail than he deigns to give (pp. 138, 146, 149) exactly what the disputes involved. We would have had a little better idea what he means by the vague term "commercial litigation." Does it include actions on notes? Actions for defective goods? And we would not have had to wonder how accurate are his frequent "estimates." In this latter regard, Nelson's methodology reminds one of the Integral Fable: A mathematician wished to test the hypothesis that every odd number was an integer. Having empirically concluded that 1, 3, 5, and 7 thus qualified, he decided to defer consideration of 9 pending development of further data. After determining that 11 and 13 were integers, he interpolated and extrapolated and pronounced the hypothesis correct.

This habit of covering factual gaps by estimation also infects some of Nelson's nonarithmetic discussion. He cites as evidence of various individuals' "lack of amenability to community controls . . . the fact that they were also indicted on criminal charges" (p. 55). Apart from his failure to describe the "charges," he seems to have forgotten that indictment does not equal guilt. He is also wrong in suggesting that jurors were drawn "by an essentially random process" (p. 36). The town clerks (then and even today) exerted an enormous direct influence on the jurors' identities.

A numerical analysis may appropriately depict other areas of the past; however, statistics—other than numbers of cases commenced and terminated, broken down, perhaps, into plainly identifiable, informative categories—do not appreciably illuminate legal history. Any examination of dispute resolution must concentrate on the facts of the disputes. The materials for such an inquiry—even in Plymouth County—are abundant. Better to have used them for description than for speculative numerology. Before we can deduce the motivation behind the choice of any particular peace-making institution, we need to know, in maximum detail, just what the differences were that required third-party resolution.

HILLER B. ZOBEL
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NORMAN FIERING. *Moral Philosophy at Seventeenth-Century Harvard: A Discipline in Transition*. Chapel Hill: University of North Carolina Press, for the Institute of Early American History and Culture, Williamsburg, Va. 1981. Pp. x, 323. \$24.00.

This book's title suggests a subject extraordinarily narrow and parochial; it turns out in fact to be remarkably broad and cosmopolitan. "Moral philosophy" may also strike the modern ear as hopelessly remote and irrelevant. Norman Fiering on the

contrary makes the case that the focus of such philosophy upon personal and public goals makes it as relevant and immediate as today's—and tomorrow's—governmental policies concerning a whole host of issues. Publications issued in a remarkable seventy-five-year period "established the foundations of nearly all modern speculation on ethics and, indeed, the basis of most of the humanitarian and democratic social and political values that are still adhered to in the West" (pp. 298–99). True, men and women of our own day do not find themselves speaking the language of moral philosophers, but once "it engaged the best minds of the time" (p. 299).

The kind of history that Norman Fiering (of the Institute of Early American History and Culture) writes is made explicit by the kind he calls for in his "Suggestions for Further Research." It is history that soaks itself thoroughly in the Latin treatises read by all educated persons in the seventeenth-century West, that examines colonial America only and always in its broad European context, that pays attention to disciplines important then (for example, logic and rhetoric) even if not so important now, and history that exploits the records of what students read and what they were taught. As the author points out, one does not learn much about seventeenth-century Americans by studying only what they wrote, for—apart from theology—they wrote little indeed. Thus to sources largely neglected by other historians Fiering has turned: "lecture notes, student commonplace books, antiquated textbooks or the student cribs taken from them, academic orations, the minutes of student societies, library lists, and so on" (p. 308).

It is by mining such sources as these that the author demonstrates convincingly that neither Perry Miller nor Samuel Morison has said the last word about Harvard or about the New England mind. They may even have said some wrong words. The quite extensive and sometimes discursive footnotes do in fact point out the shortcomings of these worthy predecessors, an especially sharp one on Miller (p. 121, n. 38) calling him to account for being "extraordinarily careless" in some of his assertions in *Jonathan Edwards*. Less explicitly, Fiering also takes issue with Joseph Haroutunian (*Piety Versus Moralism*) for the latter's failure to see that a decline of Calvinist orthodoxy was not equivalent to a decline of "the civilizing and redemptive function of religion" (p. 301).

Fiering's cast of characters, which is large, includes many familiar names: Locke, Hobbes, Shaftesbury, Hutcheson, and others. But it also includes many rescued from near obscurity: Theophilus Golius, Eustache de Saint-Paul, Franco Burgersdyck, and Adrian Heereboord—all these in chapter 2 alone. These and many more create the

"new moral philosophy," a complex amalgam drawing from Augustine, Aquinas, Descartes, Protestantism, neoplatonism, and ethical formulations of classical antiquity. One did not start from scratch (Descartes notwithstanding), but one wrestled with the question of "how best to prove and strengthen what nearly everybody already believed" (p. 300). And that was another world. In ethical theory, we have now moved into the "disintegrative stage" (p. 6).

This scrupulous book may not be received as well or read as widely as it might have been a couple of decades ago when the fashions of history were somewhat different than they are today. That would be a shame, for the needs of society do not shift as swiftly as the moods of historiography.

EDWIN S. GAUSTAD
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EDWARD M. GRIFFIN. *Old Brick: Charles Chauncy of Boston, 1705–1787*. (Minnesota Monographs in the Humanities, number 11.) Minneapolis: University of Minnesota Press. 1980. Pp. x, 248. \$20.00.

In histories of early America, Charles Chauncy of Boston makes his appearance most often as the adversary of Jonathan Edwards. Of his written works, the treatise, *Seasonable Thoughts on the State of Religion in New-England* (1743), a powerful criticism of the Great Awakening, is surely the one most often cited. The opposition of Chauncy to Edwards and references to *Seasonable Thoughts* seem to suggest that Chauncy was a "conservative," a man distrustful of emotion, perhaps a "rationalist," a moderate man memorable only for what he opposed.

A part of this commonly drawn assessment is accurate and fair. But as Edward M. Griffin's portrait of Chauncy demonstrates, Chauncy was more than an opponent of Edwards and the Awakening. He became, once the Great Awakening passed, something of a religious maverick at least in thought about universal salvation. He was not moderate in his opposition to the Anglican church. And he resembled Sam Adams and James Otis in his opposition to the British in the 1760s and 1770s.

One of the most interesting sides of Chauncy's life was the passions of his old age—his defense of the Congregational establishment and his advocacy of the American cause in the conflict with Britain. Indeed, Chauncy was more interesting in his old age than in his youth. Yet the old man for all his changes, for all his growth, had not really disavowed his youth.

Born to a distinguished line—his great-grandfather served as president of Harvard College (1654–

72)—Chauncy enjoyed a comfortable childhood. He attended Harvard, taught there, and was called to the First Church of Boston, "Old Brick," in 1727. There he preached until his death in 1787. Chauncy's life as a minister did not always flow smoothly. The controversies over the Great Awakening, the Anglican episcopate, and the American Revolution fully engaged his powers. Perhaps in part because of these struggles he grew as a man and as a minister.

If the old man looked different from the young one, he retained much of the zest and curiosity of youth. What gave Chauncy's life its unity, Griffin suggests, was Chauncy's persistent concern for the New England way. As a young man he absorbed the seventeenth-century vision of the true organization of church and state in New England and of the great mission of New England. To be sure, he gave up, or transformed, many of the ideas of the seventeenth-century fathers. Yet through these changes he continued to insist upon the special calling of the people of his land.

Griffin treats Chauncy's life with respect. He clearly admires his subject, but he is not blind to his faults; and he is not so dazzled by Edwards, whom he also admires, as to play Chauncy off against him. He has given us a solid, clear-headed, and valuable book, a book that has perspective and insight.

ROBERT MIDDLEKAUFF
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LESTER H. COHEN. *The Revolutionary Histories: Contemporary Narratives of the American Revolution*. Ithaca: Cornell University Press. 1980. Pp. 286. \$15.00.

Lester H. Cohen's study analyzes "the philosophical assumptions, ideological values, and aesthetic qualities" (p. 17) of the books by the first generation of Americans who wrote histories of the Revolution. These historians—especially John Marshall, Mercy Otis Warren, David Ramsay, and William Gordon—have not been neglected by scholars. Cohen's interest in them is distinctive because he deals less with the details of their political partisanship and more with the changes they made in the purposes and methods of writing history.

Cohen argues that the histories were intellectually as well as politically revolutionary. To explain causation they relied not on the invisible hand of God's providence but on efficacious, responsible human beings. To justify the Revolution they turned from transcendent truths of natural law to the record of events, which showed that Americans ought to retain rights that the British threatened. To narrate their stories they created a simple republican prose that did not corrupt the intellect with decadent

elegance. To preserve the Revolution for future generations they inculcated ethical principles, particularly the virtuous self-denial that must pervade a people who hoped to maintain republican institutions. To celebrate their generation's achievement they embodied its story in a heroic romance that subordinated the details of objective truth to the construction of a national "mythos" (p. 229). In short, as Cohen reiterates, writing the history of American independence was supposed to be no less revolutionary than winning it had been.

Just as Perry Miller professed to study New England Puritanism only because it provided a convenient laboratory for his larger interest in the transformation of ideas, Cohen evidently wants not only to understand his historians but also to teach his readers how to think more systematically and imaginatively. Disavowing a biographical or socio-economic approach to his subjects, he contends that "historical narratives . . . generate their own imaginative space, worlds of interpretive possibilities that transcend their immediate sphere of influence. Historians too often fail to enter that space" (p. 17). Cohen's book, then, is a case study in how to explore the intellectual universe within stories that share a common subject.

Such ambitious claims sometimes mar an otherwise cogent and valuable book when they remain unrealized in practice. Cohen elaborately demonstrates the historians' "revolutionary" abandonment of providential causation but then concludes that this change was not "a calculated process" (p. 125). Moreover, as he acknowledges in a footnote (p. 241), this uncalculated revolution in explanatory technique had been anticipated by such prewar historians as William Smith of New York and Thomas Hutchinson. He also discusses at length Noah Webster's call for an American language but devotes little space to showing this revolutionary style at work in the histories. In fact, Warren and Marshall often read like a poor man's Gibbon. Cohen rigorously exposes the historians' unspoken assumptions, murky arguments, and simplistic myths. Yet he, too, resorts to slippery metaphors—saying, for example, that a literary consciousness and a vision of the past were "a function of ideology" (pp. 151, 173). The word "function" describes mathematical relationships much more precisely than literary ones. Cohen wants not just to explicate texts but to portray a revolutionary consciousness. He cannot always get his historians to cooperate with him; at those points his highly structured analysis tends to show more rope than sail.

Whether or not it succeeds in illuminating a "dialectic" between philosophy and ideology (p. 20) or in showing that the two unite, this book will prove valuable to students of the American Revolution, American literature, and intellectual history. Cohen

ably reconstructs the ways in which the historians comprehended the American Revolution, then devised narratives that tried to make its achievements immortal. As he convincingly argues, they sought "hegemony over the future" (p. 116) by dictating how subsequent generations would perceive the nation's founding. Our understanding of their "strategies" (p. 155) is much enhanced by Cohen's lively intelligence.

CHARLES ROYSTER
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SYLVIA R. FREY. *The British Soldier in America: A Social History of Military Life in the Revolutionary Period*. Austin: University of Texas Press. 1981. Pp. xii, 211. \$25.00.

The redcoats had a mixed press in the eighteenth century. Officers publicly praised their diligence in training and their valor in combat to assure the crown that it was getting full value for its investment. Innkeepers, with a different point of view, regularly excluded soldiers and dogs from their premises.

Sylvia R. Frey deals sympathetically with the questions raised by the mixed reports. She wants to analyze the soldiers' economic and social origins and to discover why they stayed in the rigidly authoritarian, paternalistic fraternity. In six brief, topical chapters with catchy titles, for example, chapter 3, "Rewards and Recreations," the private soldier emerges from a mass of printed sources, including treatises on regimental management and drill and documentation in the Public Record Office's War Office papers. Chapter 2, "Diseases and Doctors," displays quantified data regarding national origins, years of service, age and height (labeled "size") for eight regiments. Frey observes that the soldier of 1775–82 was "a mature man of about thirty years of age, who had joined the army when he was around twenty" (p. 23). Throughout the century he was "ill-fed, ill-housed, poorly paid, uneducated, bored, diseased, depressed" (p. 70) and subjected to harsh and often brutal punishment, not to mention the prospects of death in battle. Compensations for the hardships came from membership in a cohesive community that provided "a job, however hazardous; income, however inadequate; and for some a pension, however stingy; . . . adventure, however grim; . . . and recognition, however remote" (p. 132).

Chapter 1, "Volunteers and Conscripts," offers a significant contribution to the literature. The thesis is that the rank and file were "ordinary men of modest origins" (p. 21), not criminals. Parliament authorized an impressment, which did collect soci-

ety's riffraff, but only in 1778 and 1779. Most regiments filled their ranks with volunteers, who preferred the army to starvation. They were mainly displaced and unemployed farm and industrial workers, as described by the Hammonds (*The Skilled Laborer, 1760–1832* [1927]), upon whom Frey relies heavily. Supporting evidence comes from case studies of recruitment for the elite Second (Coldstream) Regiment of Foot Guards and the Fifty-Eighth Regiment of Foot, which went to Gibraltar. It is not clear why either regiment is typical of the many sent to America or why enlistment for foreign service was better than poor relief. Nor is it clear whether sources for the Fifty-Eighth Foot covered 1775–82 or only 1759 and 1796–97, the years mentioned in the text and notes. The relevance to 1775–82 of recruitment during 1796–97, when England and the fighting services experienced dark days, is equally unclear. The problem, stated most generally, is the casual use of quantified information in a narrow context.

Another problem is secondary sources. The author appears unaware of many excellent regimental histories, recent articles in sociology and psychology, and William B. Willcox's 1962 study of Sir Henry Clinton. In view of John Cuthbert Long's 1933 biography of Lord Jeffrey Amherst, the assertion (p. 146, n. 2) that he remains to be studied is indeed curious.

Frey's article, "The British and the Black: A New Perspective" (*The Historian*, 39 [1976]: 117–31), showed excellence in research, exposition, and insightful content. By comparison, *The British Soldier in America* is a lesser piece.

W. KENT HACKMANN
University of Idaho

F. DE BORJA MEDINA ROJAS. *José de Ezpeleta: Gobernador de la Mobila, 1780–1781*. (Publications de la Escuela de Estudios Hispano-Americanos de Sevilla.) Seville: Escuela de Estudios Hispano-Americanos. 1980. Pp. lxxxii, 869. 3,000 ptas.

This long, detailed narrative is a significant contribution to the growing literature on colonial Gulf Coast history. Specifically, the author describes the activities of Governor José de Ezpeleta y Galdeano in Mobile from May 1780 to the fall of British Pensacola in May 1781. Despite Ezpeleta's illustrious career—as captain general of Louisiana and West Florida, captain general of Cuba, viceroy of New Granada, captain general of New Castile, governor of the Council of Castile, twice captain general of his native Navarre and once of Cataluña—the author, F. de Borja Medina Rojas, has focused on Ezpeleta as governor of frontier Mobile, where the talented Navarrese began his meteoric rise in the ranks of the Spanish bureaucracy.

As governor, Ezpeleta had two responsibilities: (1) to maintain the Spanish foothold in Mobile and (2) to aid Bernardo de Gálvez in his assault on Pensacola in 1781. The first task was difficult. Ezpeleta had to face the opposition of British and French residents in his jurisdiction, attacks by British forces garrisoned at Pensacola, and raids of hostile Indians of English persuasion. Shortages of men, food, arms, munitions, and boats also hampered his efforts to strengthen the Spanish position in the Mobile area. Still, Ezpeleta succeeded because of his superior leadership qualities, timely help from Spanish authorities in Cuba and New Orleans, and the defection or undependability of Britain's Indian allies. In 1781 he got the opportunity to distinguish himself even more by preparing and leading the land march that set up the beachhead in the bay of Pensacola for landing the Spanish expeditionary force. He also served as major general of the expedition and as interim commander when Gálvez was out of action because of wounds to his hand and abdomen. More cautious and deliberate than Gálvez, Ezpeleta did not always agree with the audacious tactics designed by his superior, which put his men in jeopardy, but as a good officer Ezpeleta followed orders. His rewards were the highest royal offices and titles to be garnered both in the Indies and the metropolis.

Developed in fulsome detail (the author averages about three pages per day for the Ezpeleta governorship), this book meticulously documents the day-to-day events on the Gulf Coast from Ezpeleta's vantage point. But the minute descriptions are neither tedious nor excessive. The author's intensive research in American, English, French, and Spanish archives and his view of the Gulf Coast as a geophysical area where the crucial factors were geography, diet, transport, disease, and the interaction of whites, blacks, and Indians give the narrative a multidimensional quality. Moreover, fifty-one reproductions of eighteenth-century maps and drawings provide a valuable visual framework of this Gulf Coast milieu. In sum, this is narrative history at its best; Ezpeleta's governorship in Mobile has found a definitive interpreter.

JOHN J. TEPASKE
Duke University

RONALD HOFFMAN and PETER J. ALBERT, editors. *Diplomacy and Revolution: The Franco-American Alliance of 1778*. Charlottesville: University Press of Virginia, for the United States Capitol Historical Society. 1981. Pp. xii, 200. \$13.95.

The five fine essays included in this volume were presented at a conference celebrating the bicentennial of the Franco-American treaties of 1778 in

Washington, D.C., on May 15–16, 1978, sponsored by the United States Capitol Historical Society. The result is a neatly coordinated and balanced summary of contemporary scholarship on the subject.

Alexander DeConde opened the program with a masterly review of "The French Alliance in Historical Speculation." He found the conventional interpretations generally sound, with some reservations relating to "especially their ethnocentricity" (p. 36). DeConde believes that the effective life of the alliance was just five years: that was enough to ensure American independence.

In an essay entitled "American Views of France in 1782: Public Reaction to Yorktown and the Birth of the Dauphin," William C. Stinchcombe asserts that the celebration of the Franco-American victory at Yorktown in 1781 and of the birth of the French dauphin in 1782 "shows us how far the Americans had come in acknowledging the influence of the European world on their own affairs, in contrast to the narrow isolationism of 1774 and 1775" (p. 71).

Two of the essays concentrate on French foreign policy. Jonathan R. Dull writes of "France and the American Revolution Seen as Tragedy." With the benefit of hindsight, he pronounces French participation "a failure of tragic dimensions." Vergennes failed in his goal of weakening Great Britain in order to persuade that proud nation to cooperate in the maintenance of the European balance of power because he understood only classical diplomacy. Ironically, Dull finds the material and psychological costs of the war greater for France and the United States than for Great Britain. Orville T. Murphy looks more closely at Vergennes's foreign policy in "The View from Versailles: Charles Gravier Comte de Vergennes's Perceptions of the American Revolution" and comes to conclusions similar to Dull's. Vergennes believed that a proper European balance could be maintained only with France as arbiter, a position, in his view, that England had usurped. Vergennes, he says, "deliberately provoked a war with England to recover for France a role in European affairs that had been presumably lost in 1763" (p. 120).

The final essay, by Lawrence S. Kaplan, is a comparison of "The Treaties of Paris and Washington, 1778 and 1949: Reflections on Entangling Alliances." Reviewing the histories of the two alliances, Kaplan concludes that small nations in alliance usually believe that their more powerful partners use them for their larger purposes. So the Americans with the French alliance of 1778, but on balance, he says, "the smaller power entangled the larger in an alliance that served American interests far more than it served the French" (p. 194). Noting that France in the North Atlantic alliance has behaved most like the United States in the eighteenth

century, he wonders whether the history of NATO will be the same.

ALBERT HALL BOWMAN
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Chattanooga

GARRY WILLS. *Explaining America: The Federalist*. (America's Political Enlightenment.) Garden City, N.Y.: Doubleday. 1981. Pp. xxii, 286. \$14.95.

Gary Wills's study of *The Federalist* is the second of a projected series of four volumes that will be entitled *America's Political Enlightenment*. The first volume, popularly acclaimed and winner of a number of prestigious prizes, was an innovative analysis of Thomas Jefferson's Declaration of Independence. Succeeding volumes will deal with the Constitution and the Supreme Court.

The major thesis of *Explaining America* (there are dozens of minor ones on comparatively minute points) is that James Madison and Alexander Hamilton, the principal authors of *The Federalist*, were decisively influenced by the Scottish Enlightenment, notably David Hume's essays. Particularly was this true of Madison to whom the major and a disproportionate part of this study is devoted. Impressed by the scholarship of the late Douglas Adair, a distinguished student of Madisonian thought whose doctoral dissertation of 1943 (largely unpublished) argued that Madison's intellectual development was heavily indebted to the Scottish Enlightenment, Wills proposes in this book to show that Hume's influence on the Virginian was far greater than even Adair realized. "Madison's thinking," Wills writes in a characteristic passage, "had a Scottish accent from the start" (p. 23).

As with Madison, so with Hamilton. To prove the New Yorker's heavy reliance on Hume, however, Wills principally relies on a detailed analysis of Hamilton's well-known speech of June 18, 1787, before the Constitutional Convention, which in Wills's view was permeated by "a Humean ambience" (p. 83). Wills then proceeds to argue (with considerable dubiety) that there was an inseparable link between the latter and Hamilton's Federalist essays.

Madison (especially in his tenth "Publius" essay) was indisputably indebted to Hume, but in view of the Virginian's wide-ranging study and erudition I do not believe it plausible to contend that the influence of one writer could have been quite as pervasive and decisive as Wills alleges. Apropos of Hamilton, I am confident that Wills exaggerates the effects of the Scottish philosopher's essays. Although the New Yorker certainly read and sometimes quoted Hume in his other writings, there is no convincing basis for the assertion that as "Publius"

Hamilton was decisively influenced by the Scotsman. His principal guide was his own experience and his extraordinary legal and logical skill in presenting the best possible case for his client, in this case the United States Constitution.

How does Wills contrive to find the hidden hand of Hume in virtually every important argument set forth by "Publius"? The answer is that he selects from *The Federalist* and from Hume those passages that buttress his thesis and ignores sections (and essays) that might refute it.

The most original feature of Wills's monograph is his convincing refutation of Adair's discovery of "a schizophrenic Publius" (p. 73), of an ideological split between Madisonian and Hamiltonian political philosophy. In dismissing the stereotyped view of Madison and Hamilton's contribution to *The Federalist*, Wills maintains that on substantive issues (the necessary and proper clause, the taxing power, broad versus loose construction, state versus national sovereignty, checks and balances, trust in the people—Wills contends that neither took the dismal view of human nature customarily ascribed to them) the Virginian often speaks like the stereotyped Hamilton and the New Yorker like the putative Madison. "We are left," Wills writes in summary, "not with a schizophrenic Publius, but with two men each separately schizophrenic, giving us *five* Publii—Jay; the Madisonian Madison, and the Hamiltonian one; and the Hamiltonian Hamilton, as well as the Madisonian one" (p. 78). It is an instructive and plausible insight.

The final section of Wills's treatise (twenty-one of the book's thirty-one brief chapters) is given over to a detailed discussion of "checks and balances" and "representation," which consists largely of a microscopic examination of Federalist essays 10 and 51. Once again, one encounters an example of Wills's extraordinarily narrow selectivity. Here, his argument consists largely of abstruse speculations on political philosophy (the idea of mixed government as drawn from Aristotle's *Politics* and the *Histories* of Polybius, for example), textual exegesis (focusing often on the meaning of a sentence or a word), and of a running dialogue with other close students of *The Federalist*—notably Robert Dahl (*Preface to Democracy*).

The less said of Wills's "epilogue" the better. His argument is, in effect, that *The Federalist* must be understood in the context of its own day, not ours, that Madison (and presumably Hamilton) were "observing a world different from ours, living in that world responding to it. And much of that world has disappeared or been so changed to mislead more than it would have by simple disappearance" (p. 268). Who would disagree with such truisms?

In sum, *Explaining America* will presumably be of

scant interest to the general reader and of limited utility to most historians.

JACOB E. COOKE
Lafayette College

ALLAN S. EVEREST. *The War of 1812 in the Champlain Valley*. (A New York State Study.) Syracuse, N.Y.: Syracuse University Press. 1981. Pp. viii, 239. \$15.00.

If the hopes of the War Hawks had been realized, the Champlain Valley would have been the scene of dramatic offensive warfare in the War of 1812. An enthusiastic American army would have struck northward, brushed aside British resistance, and taken Montreal. This was not to be. A divided country and an ill-prepared army encountered only frustration along the main invasion route to Canada. The ineffectiveness of American efforts along the Champlain Valley presents definite problems to a historian who wishes to write of the region in the War of 1812, for the campaigns of 1812 and 1813 did not amount to much. Only in 1814 did the Champlain Valley assume its expected importance in the war, and then in a very different manner from that expected by the War Hawks; defense not offense became all important.

Allan S. Everest has not overcome all the difficulties presented by the uneven pattern of events along the Champlain Valley in the War of 1812, but he has written a competent account of the military preparations and actions both in the United States and Canada. The book is more concerned with the main American-British confrontation in the region than it is with the internal history of Vermont and New York. Everest presents this confrontation in a balanced manner and shows an understanding and appreciation of the Canadian as well as the American situation. The climax of the book is Thomas Macdonough's dramatic naval victory on Lake Champlain in September 1814, but most of this history is concerned with less dramatic events; with the efforts to gather American forces in the region and to plan offensives, with Henry Dearborn's tentative and useless advance northward in the fall of 1812, and with Wade Hampton's slightly more bloody but equally useless endeavors in the fall of 1813.

The main omission in Everest's book is a more detailed examination of the political and economic situation in Vermont and New York. Some space is devoted to these states, but more could have been done, for example, to help explain why Vermont, with strong links to Canada, gave support to the Republican party and the war in 1812. The main outlines of the military campaigns of the War of 1812 are already well known but regional problems

connected with the war have received far less attention. If Everest had chosen to delve more deeply into the situation in the states of the Champlain Valley his book would have had the possibility of a freshness that is difficult to attain when discussing the history of the main campaigns.

REGINALD HORSMAN
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ALEINE AUSTIN. *Matthew Lyon: "New Man" of the Democratic Revolution, 1749-1822*. University Park: Pennsylvania State University Press. 1981. Pp. xii, 192. \$16.50.

Two events serve to separate Matthew Lyon from the ranks of third-rate legislators whose records make them more fit to be fodder for the quantifier than to be subjects for a full-length biography. The first established something of a record for "gross indecency": Lyon spat upon the face of fellow congressman Roger Griswold. For this crass behavior the House did not expel him, although Griswold took personal revenge two weeks later by repeatedly bashing Lyon with a hickory stick until he was thoroughly bruised and bloody. The significance of the battle is unclear, except to show that low-class Irishmen may be uncouth whereas elite Congregationalists use weapons that do real harm, but it is a story widely told to awaken eager college students to the joys of historical inquiry.

The second event was Lyon's indictment, trial, and ultimate imprisonment as a "malicious and seditious person . . . of depraved mind and . . . diabolical disposition" (p. 110) who had violated the Sedition Act on three counts. Lyon surely was a victim of this repressive piece of legislation, one that historians too easily deplore as an unfortunate excess rather than analyzing how the political theory that brought forth the Constitution within a decade evolved logically to a position so alien to the rights of free citizens. From his jail cell Lyon waged a successful campaign for re-election, a harbinger of Republican victories ahead but apparently not fully a personal triumph; in May 1801 he left Vermont forever to build a new life on the Kentucky frontier.

Aleine Austin's book is a welcome addition to the historical literature on the Revolution and the early national period. It begins by placing Lyon among the "new men" of the late eighteenth century, opportunistic individuals of humble origin who rose by their own efforts to solid "middle-class" status and ultimately shoved aside the ruling Federalist elite. Lyon was so eccentric, and the "new men" hypothesis found in the works of Alfred Young, Gordon Wood, Jackson Main, James Martin, and Paul Goodman is sufficiently ambiguous, that it is

just as well that the remainder of Austin's concise biography sticks mostly with the facts of Lyon's career and interprets them cautiously and only occasionally. Perhaps because of the quality of extant documentation, the account is more detailed for Lyon's early years as a speculative landholder, occasionally dishonorable Revolutionary officer, entrepreneur, Vermont separatist, perennial candidate for office, and two-term congressman than for his later years as a Kentucky slaveholder, merchant, and commissioner general of the Western Army. He returned to the House for an eight-year stint, ended in 1811 by the will of his southwestern constituents who identified him with Federalist opponents of the War of 1812, but to these less controversial years of service the author devotes just four pages. The treatment of Lyon's duplicitous connections with James Wilkinson and Aaron Burr is hurried and drawn mostly from secondary sources.

The "Conclusion" is not a conclusion at all, but a wholly new psychological sketch that portrays Lyon as struggling to overcome a well-founded sense of inferiority by becoming a member of the elite that had wronged him in his early, formative years. In this he succeeded, although he never fully resolved his ambivalence toward his new role; despite years of dedicated humanism and public service, he remained at the same time a self-serving egotist. Since the author chose not to revise the body of her doctoral dissertation to reflect this new insight, readers may be at a loss to evaluate its merits; yet it should not be ignored.

RUDOLPH M. BELL
Rutgers University

DUMAS MALONE. *The Sage of Monticello*. (Jefferson and His Time, number 6.) Boston: Little, Brown. 1981. Pp. xxiii, 551. \$19.95.

The public attention that the publication of the sixth and final volume of Dumas Malone's monumental biography of Jefferson has received is well merited, and its scholarly recognition is equally deserving. It is the pre-eminent Jefferson biography of our time and will endure as one of the great accomplishments in American biography. In producing the work—the first volume of which appeared in 1948—Malone has displayed almost as much patience and persistence as Jefferson did in building Monticello. Like Jefferson he also has enjoyed a long and active life. Although he first contemplated writing a comprehensive life of Jefferson as a young professor at the University of Virginia in 1926 (then a bold approach, for Jefferson was still largely studied as a political figure), he did not begin writing it until 1943; and when he retired from Columbia in 1959,

only two volumes of what had originally been projected as a four-volume work had appeared. The author knew by then that four volumes would not contain a work of such broad scope. When Malone closed out Jefferson's public career at the end of volume five, that impressive achievement was recognized with the Pulitzer prize for history in 1975.

We are in Malone's debt for not resting on his laurels but pushing on, despite failing eyesight, to complete his task (here the able assistance of Steven H. Hochman, his research assistant, should be recognized). In the final volume we are rewarded with some welcome summing-up and reflective comments about Jefferson that reveal Malone's long journey with his subject. There is no summation as such, but since Jefferson in retirement frequently looked back on his earlier experiences or reacted to current happenings in the light of his own past, his biographer also has an opportunity—which Malone employs effectively—to engage in similar reviews and reflections. Many of these comparisons are instructive. Jefferson's ideas did change, and in old age he did become more conservative in some things, especially economic matters. His fears about national consolidation and commercialization were obsessive; and the nation could hardly have endured had the extreme doctrine of states rights that he approved of in his later years prevailed. At the same time, his ardor for self-government increased with the passing years; he wanted more local democracy, and he continued to believe that each generation had the right to revise its government in the light of change and experience.

Jefferson was nearing his sixty-sixth year when he finished his second term as president in 1809 and retired to his family, his farms, and his books. He died at Monticello on July 4, 1826, without having left his native state during his seventeen years in retirement. He kept up with public affairs, was frequently overburdened with visitors to Monticello, and continued his extensive correspondence—and in one of his most important acts reopened contact with John Adams—but after leaving the presidency he did not again involve himself in political affairs. In 1815 at the age of seventy-two he turned over the management of his land in Albemarle County to his eldest grandson, Thomas Jefferson Randolph, and in the same year sold his library of over 6,000 books to Congress. But even then he did not really retire. He said he could not live without books and immediately began to assemble another library.

The promotion of education in his native state soon became one of the driving forces of his last years. Though he would have no more success in the promotion of a comprehensive system of public schools than he had had in earlier years, he triumphed in the establishment of the University of

Virginia. Malone sees Jefferson rendering his most memorable public service in retirement as an advocate of enlightenment, and his crowning achievement was the University of Virginia. Malone is particularly effective in showing how Jefferson pushed ahead with his plans despite the lack of legislative support and at times against open opposition. Few saw Jefferson's vision, and a less determined man would soon have abandoned the struggle. Besides, probably only a man of his stature could have got away with the independent actions he sometimes took. Malone's account is admiring, but not uncritical. He does not hesitate to point out that the founder wanted the board of the university to make sure that no doctrines incompatible with the basic principles of the state and federal constitutions were inculcated. Malone writes appreciatively of the architectural masterpiece that Jefferson designed for his "academical village," and it was fitting that the University of Virginia and the Thomas Jefferson Memorial Foundation honored Malone upon the completion of his biography with a dinner in the dome room of the Rotunda, where at the first dinner in that magnificent room, Jefferson and his neighbors honored Lafayette, whose famous visit to Monticello in 1824 Malone movingly describes.

As might be expected, we see more of the human side of Jefferson in this volume than in the earlier ones. Jefferson was the center of an extended family circle that included a dozen grandchildren, nieces and nephews, and various in-laws, many of whom lived at Monticello for extended periods of time. Malone is particularly adroit in showing Jefferson's family relationships and the close ties that he developed with some of his older grandchildren, especially Thomas Jefferson Randolph upon whom he came to depend heavily. His family brought Jefferson great joy but also many tribulations, which he faced with characteristic equanimity.

The greatest distress caused him in his last years came from his worsening financial situation. Malone devotes considerable attention to clarifying Jefferson's tangled financial affairs and the process by which he slipped deeper and deeper into debt. His judgment is that factors beyond Jefferson's control were primarily responsible for his ultimate insolvency. The story is a moving and tragic one that destroyed the tranquillity of Jefferson's declining years and necessitated the sale of most of his property after his death.

Malone's Jefferson is a human and humane man who did not accomplish everything that we might have wanted him to, and this is especially true in regard to slavery. But he may have accomplished as much as it was possible for one person to achieve in his place and age. His biographer's closing tribute is that "he perceived eternal values and supported

timeless causes. Thus he became one of the most notable champions of freedom and enlightenment in recorded history" (p. 499).

NOBLE E. CUNNINGHAM, JR.
University of Missouri,
Columbia

J. LEITCH WRIGHT, JR. *The Only Land They Knew: The Tragic Story of the American Indians in the Old South*. New York: Free Press. 1981. Pp. xi, 372. \$16.95.

Apart from an occasional great leap forward into the present, this volume speaks mainly of southeastern tribal populations in the sixteenth, seventeenth, and eighteenth centuries. Boldly, imaginatively, and with high black humor, J. Leitch Wright, Jr., unfolds the implications of the demographic revisionism of the past two decades.

On the eve of the European entry, powerful native confederacies, whose total populations numbered well over a million, lived in large village groups along coasts and rivers from the Gulf of Mexico to the Chesapeake. During the sixteenth century, Spanish soldiers, Franciscan missionaries, and shipwrecked sailors of many nations familiarized natives with European artifacts and customs and initiated shock-waves of demographic disaster. Africans and Europeans contributed microparasites that literally wiped out some tribes, decimated others, and fatally weakened the social and political cohesion of many groups that remained. In their incessant search for laborers among the dying populations, both Spanish and English traders and warriors involved native partners in greatly intensified warfare against animal friends and human enemies. Hides, furs, and especially slaves, were the aim of the game.

Wright argues that the extent and impact of native American slavery in the Southeast has been grossly underestimated. While captive Indian students could, and often did, run home to their kin, slaves from distant battlegrounds could find no succor among neighboring tribes who had, in fact, initially captured and helped enslave them. Indian slaves proved unsatisfactory only in their tendency to succumb to the diseases of their masters and of their African fellows. When they succumbed, however, the masters contrived to draft replacements from tribes yet more remote. Hence, Wright argues, the southeastern American "Negro" population and culture represents an amalgam of African, native American, and white elements. The Indian contribution to this amalgam, both cultural and genetic, is far greater than most have supposed.

In the context of these findings, the celebrated Indian "removals" of the nineteenth century take on new implications. The tribes Jackson harried west had enjoyed almost two generations of relative

stability and growth—an experience almost unparalleled among native populations in close contact with whites and Africans. The horrors of removal represent not an aberration, but a return to the norm—with one important exception. Unlike the Cofitachiqui "kingdom" or the Yamassee "tribe," the Choctaw, Creek, Chickasaw, Cherokee, and Seminole "nations" did not disappear; and their populations were not, literally, "decimated." By contrast with their seventeenth- and eighteenth-century British and colonial predecessors, Thomas Jefferson and Andrew Jackson may appear every inch the "humanitarians" they claimed to be. Of course, the great "civilized" Indian nations had had some centuries to develop relative immunity to smallpox by the time Jackson's agents got around to vaccinating them.

Wright substantiates his far-reaching hypotheses with impressive documentation from American, British, Spanish, Mexican, and Caribbean archives. Since the Cofitachiqui and the Yamassee left no such abundance of written commentary as the Chickasaw and the Cherokee, much of what he concludes involves conjecture and may be debated. For once, however, the book jacket is right on target. This is, indeed, "a must-read book for anyone interested in American history."

MARY YOUNG
University of Rochester

NICHOLAS P. HARDEMAN. *Shucks, Shocks, and Hominy Blocks: Corn As A Way of Life in Pioneer America*. Baton Rouge: Louisiana State University Press. 1981. Pp. xii, 271. \$20.00.

In spite of what Nicholas P. Hardeman says, corn is not, and never was, a way of life. Corn is a plant, and a way of life is something else. Even as a figure of speech the phrase does not ring true, and the hyperbole is clearly unnecessary. Corn is indeed the most useful and amazing plant known to humans. It has a seed-yield ratio unequaled by any plant anywhere and has a climatic range without parallel. It did indeed sustain those who conquered a continent. As direct human food, however, corn was always the crop of poverty or necessity, and was superseded by other foods as soon as people could do without it. Roasting ears are, of course, a delicacy eaten for fun. They are, however, by no means as essential to survival as corn was to the pioneers, or as it was and is to livestock raisers.

The story here recounted concentrates on pioneers almost to the exclusion of anyone else. Only a few lines deal with the more settled parts of America and with the use of corn by urbanites. There is also the trendy, and almost obligatory, lament about modern science and technology that has eliminated

most of the drudgery associated with corn husbandry.

The author fully and accurately describes every aspect of corn growing and processing, from planting to harvesting and beyond. The book is arranged topically, with the usual concomitant overlapping of time periods. More than that, the author seems to have little regard for chronology, skips back and forth across time, and often forgets to give even general dates. When, toward the end of the book, the author gets around to modern corn and to modern commerce in the plant, he is uneven in his presentation and mostly lets the old nostalgia hang out. For those involved with period farms, the book is invaluable. It most clearly details exactly how a shock is constructed, corn shucked, or hominy made.

For those unfamiliar with the history of the westward movement in America, the book is doubly rewarding. The author explains land laws, land tenure, timber clearing, transportation, and a host of other things. All are related to corn importantly, but indirectly. Those who already know a little about the American frontier may find much of the book redundant. For the most part, his narrative ends about 1900.

There are no footnotes, but that is easily forgiven. The book is based on a tremendous amount of research, and inserting footnotes would have delayed publication by another five years, most likely. The illustrations are wonderful. They consist mostly of line drawings by Linda M. Steele. Read this book. It is marvelously informative, even when it overstates its case.

JOHN T. SCHLEBECKER
Smithsonian Institution

DAN SCHILLER. *Objectivity and the News: The Public and the Rise of Commercial Journalism*. Philadelphia: University of Pennsylvania Press. 1981. Pp. xiv, 222. \$20.00.

In the early 1830s when skilled workers in large eastern cities earned less than \$10 a week, subscriptions to the commercial press cost \$8 to \$10 a year. Thus journalists and printers in Boston, New York, Philadelphia, and Baltimore realized the potential market for cheap newspapers. The publication of Benjamin H. Day's *New York Sun* on September 3, 1833, began a new era in American journalism. The *Sun*, the first of the penny press, aimed for a readership unable to afford the old commercial newspapers. Sold on the street corner rather than by subscription, from the beginning penny papers emphasized sensational crime and sex stories to capture interest. As these newspapers needed both large circulation and high advertising revenues to

survive, they had to serve a politically heterogeneous public. In this demand, Dan Schiller finds the genesis of the concept of news objectivity.

Schiller says historians of journalism have missed the direct connection between the penny press and the labor press of the 1830s. The labor press had attacked the commercial press for its failure to present objective news. The labor press believed that knowledge could lead the workingman to a proper exercise of the ballot, thus reclaiming his rights and liberties. The commercial press denied him knowledge because it served special interests. Although the labor press, patronized by trade unions and subscriptions of artisans and mechanics, did not survive the panic of 1837, it prepared the way for a journalism that eschewed special interest. The penny press "appropriated and softened" (p. 46) the message of the labor press, making itself the instrument of enlightenment and political independence. Schiller points out that a softening of the rhetoric also marked a change in objectives. The labor press called for an impartial presentation of news to redress injustice and achieve social reform. The proprietors of the penny press promoted the public good through publication of "the facts" to increase circulation and profits.

To illustrate how newspapers' content and form continually asserted their objectivity, Schiller uses the example of the *National Police Gazette*. A commercial venture, designed to make a profit for its owners, the *Police Gazette* appealed to public interest in crime news. Like the penny press, it claimed objectivity as its primary goal, buttressing this with constant assertions of its accuracy. Schiller says that specialization itself became "a feasible defense of validity and reliability" (p. 107). Specialization made the *Police Gazette* the authority. The newspaper also verified its authority by telling the reader where information had been obtained, how reliable it was, and what it meant. First person stories of criminals told in underworld argot also confirmed authority and objectivity. Finally, the *Police Gazette* appealed to officers of the state to verify contents of news accounts. To Schiller, this appeal to authority becomes a major paradox in a system of objectivity justified as a protection of the public interest: "commercial journalism's periodic account of reality would systemically echo the judgments and perspectives of the power elite" (p. 123).

Schiller offers a useful critique of the development of objectivity and the news. He has rightly pointed out that the concept developed as a commercial strategy and that it is largely self-validating. His conclusion that the American people must become "lord of the facts" is commendable. Unfortunately, we are not told how.

DONALD W. CURL
Florida Atlantic University

THOMAS DUBLIN. *Women at Work: The Transformation of Work and Community in Lowell, Massachusetts, 1826–1860*. New York: Columbia University Press. 1979. Pp. xiii, 312. \$17.50.

This study of workers in the early New England textile industry is based solidly on records of the Lowell mills, including records of the Hamilton Company, directories and federal census schedules, local histories and genealogies, memoirs and collections of letters, and the extensive secondary literature on the subject.

Thomas Dublin traces the growth of the early textile industry, the founding of Lowell in the 1820s, and recruitment of workers from among young women of rural New England. He goes further than earlier writers in tracing the social origins and motivations of these women and in placing their mill experience within their broader life cycle. He shows that they came from the broad middle ranges of wealth in their home towns, that they came to Lowell as members of broad kinship networks, and that they spent and saved earnings as they chose. Dublin poses the important question, "Did Lowell make any difference in their later lives?" In terms of their marriage ages, their husbands' vocations, and where they later settled, he concludes that, for more than a third of them, work in Lowell "constituted an entry into the urban industrial world and signaled a permanent departure from the one in which they had grown up" (p. 54).

The author then examines the close-knit worker community in Lowell, the significance of its solidarity in the growth of the protest movement of the 1830s and 1840s, and the changing consciousness of these women operatives. He finds that the constant innovation in the technology and organization of production resulted in "a socialization of work" and greater solidarity among women operatives (p. 60). Furthermore, almost three-quarters of the women employed by the Hamilton Company in 1836 lived in company-owned housing, principally boarding houses, which were centers for the process of socialization. This process was not limited to matters of speech or church attendance. "Group pressure to conform . . . played a significant role in shaping their collective protests" (p. 85).

The protests of the 1830s and 1840s were over wage cuts and an accelerating work pace, which were seen by these "daughters of freemen" as attacks on their heritage and their economic independence. The agitation of the mid-thirties was dampened by the depression of 1837–42, but with recovery came a new female labor movement. For most of the decade there was agitation for enactment of a state ten-hour-day law. In the course of this struggle, "women activists came to a new

sense of the 'sisterhood' of working women and developed a critique of the 'cult of true womanhood . . .'" (pp. 125–26).

Dublin's later chapters deal with the transformation of work and the labor force in the Lowell mills and the decline of labor protest. Increased competition in the industry brought pressure for lower prices and wages and higher productivity, which combined with demographic changes to transform the work force. As recruitment of Yankee women slowed, the appearance of the famine-fleeing Irish in New England offered an alternative source of labor. By 1850 one-third of the workers at the Hamilton Company were immigrants, and "a substantial majority of the work force resided in private housing" (p. 143). Although the new Irish workers lacked the independence of the Yankee women (and were resented by them), and although they experienced discrimination in job placement, pay, and housing, their position in the Lowell mills improved significantly in the 1850s, as the Yankees' advantages diminished. The most striking development was the new residential diversity and dispersal that "removed an important element in the shared experience of women workers" (p. 167).

One can find problems with this study. Dublin struggles unnecessarily with the question of Lowell's "representativeness." There is much repetition. To this reader, the point of the later statistical analyses is not always clear. In analyzing income differences over the years, Dublin does not attempt to adjust for changes in the cost of living. His categorizing workers aged sixteen to twenty-two as "children," despite his explanation, remains puzzling. As evidence of a leveling of wages in the 1850s, he shows that there were newcomers in all mill departments in 1860, in contrast to their concentration in relatively unskilled, low-paying departments in the 1830s, but it is not clear that the newcomers in 1860 were inexperienced workers. His account of Irish operatives and their values is thin compared with that of the Yankee women. It is doubtful that, whatever their famine experience, the Irish were as content with their life in Lowell as he suggests they were initially.

Nonetheless, this is an important study, and its problems are far outweighed by its merits. Dublin's careful, balanced analysis enriches our knowledge of these much-studied workers. He has pushed his account of Lowell and its work force significantly further and deeper than earlier accounts and has shown in considerable detail the historic transition in the work force and conditions. His study also contains an instructive series of appendixes explaining his methodology. For all this, students of the New England textile industry, labor movements, and women's history can be grateful.

JOHN BORDEN ARMSTRONG
Boston University

BERNARD CRESAP. *Appomattox Commander: The Story of General E. O. C. Ord*. New York: A. S. Barnes. 1981. Pp. xii, 418. \$15.00.

Strangely, all the biographical activity focused on Civil War generals in the past two decades has only now produced a biography of Union General Edward O. C. Ord. He played a significant part in the early campaigns of the western theater, especially Vicksburg, then came east to distinguish himself in the Virginia campaigns. Succeeding Ben Butler as commander of the Army of the James, he became the third-ranking Union general (after Grant and Meade) in the Petersburg and Appomattox operations. He won plaudits for his seizure of Fort Harrison, a formidable Richmond defense work, and for the decisive move that placed his army athwart Lee's escape route from Petersburg and brought him to bay at Appomattox. He figured prominently in the tense truce negotiations that climaxed in Wilmer McLean's parlor, and appropriately he looked on as Grant and Lee signed the historic accord ending hostilities.

After the war, Ord had an unhappy experience as a Reconstruction commander—he was too moderate for the radicals and not moderate enough for the conservatives. Then he moved on to more congenial service on the Indian frontier of the West. Early in his career, before the Civil War, he had performed admirably as a company commander in western Indian campaigns. Now he returned to the West to spend his final years of active duty as a department commander. His most notable tour was in Texas during the 1870s, an assignment that involved delicate relations with Mexico. The Texans loved Ord, but even Sherman's friendship could not save him from a politically motivated forcible retirement at age sixty-two in 1880.

The Ord that emerges from these pages is a physically robust and courageous officer, full of vitality, quick thinking and alert, skilled strategist and tactician, and able leader of men. In battlefield performance, he rose considerably above most of Lincoln's generals. By contrast, his political and bureaucratic judgment tended to be erratic and his temperament mercurial. He greatly harmed his professional fortunes by such thoughtless actions as opening peace talks with Confederate General Longstreet in front of Petersburg, exploiting his friendship with General-in-Chief Sherman while Texas commander to short circuit his immediate superior Sheridan, and endorsing Democrat W. S. Hancock in the presidential campaign that James A. Garfield won.

Bernard Cresap (who probably is related in some way to Ord, one of whose middle names was Cresap) does a competent, objective job of chronicling Ord's career, probing his character and per-

sonality, and evaluating his significance. The research is thorough, especially in the rich Ord papers, and the presentation sufficient without being engrossing. This is a useful addition to the literature of the Civil War and Reconstruction as well as of the Indian frontier.

ROBERT M. UTLEY
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MICHAEL B. CHESSON. *Richmond After the War, 1865–1890*. Richmond: Virginia State Library. 1981. Pp. xxi, 255. Cloth \$20.00, paper \$12.50.

Reading *Richmond After the War*, one senses that during a large part of the quarter of a century ending in 1890 the capital of the Old Dominion stood irresolutely at the doorway to greatness but failed, perhaps feared, to cross the threshold. Antebellum Richmond, Michael B. Chesson suggests, possessed, by Southern standards, an exceptional industrial base and almost unparalleled transportation (especially railroad) facilities. But in an era when manufacturing and rail access were the key elements in urban growth, the city, even when expanding its enterprises, fell farther and farther behind. The question that underlies Chesson's multifaceted examination of "a city that failed" (p. xviii) is, "Why?"

The temptation is strong to join many Richmonders in attributing the city's relative decline to the physical destruction of the last days of the war and the crushing burden of Reconstruction. It is probably for that reason that Chesson devotes some two-fifths of his study to the period 1865–70, for he sees here many myths to be dispelled and much need for honest appraisal. He is at some pains to disprove the assertions that the military government was tyrannical or that the city government was at any time dominated by blacks and also strongly challenges the assumption that the Reconstruction era constituted a significant break in the city's governance structure and attitudes. While acknowledging—indeed, treating in some detail—the destruction that accompanied the Confederate evacuation of the capital, Chesson, in his last two substantive chapters on the 1870s and the 1880s, clearly implies that the economic decline of these years cannot be attributed to those earlier losses.

Richmond After the War explores a wide array of urban elements, ranging from reigning belles to sewers, from religion to crime, and from streetcars to cemeteries. There are, however, several major components of the city's life that Chesson examines at some length in each of the five chronological sections of his study—the antebellum era, the Civil War, the Reconstruction period, the 1870s, and the 1880s. These elements are: the city government, manufacturing, transportation, and the black popu-

lation. He perceives some shifts (hardly major, except for the addition of black councilmen from Jackson Ward) in the types of persons serving on the city council; considerable continuity in their attitudes toward city services; ultimate decline after an early recovery in both industry and transportation facilities; and, despite the continuing growth of black institutions and the black sense of community, a steady erosion of black political power, resulting largely from increasingly effective white efforts to reduce the size and importance of the black vote.

There are some weaknesses in Chesson's analysis and presentation of these and other elements of Richmond's history. The method of identifying both black and Republican leaders is extraordinarily loose and unstructured. Both voting analysis and the analysis of the city council membership are distressingly limited and not always clearly presented, especially before 1871. Railroad maps are badly needed in the discussion of transportation developments, and the treatment of manufacturing statistics is less rigorous than one might wish.

Richmond After the War is, nevertheless, a solid and commendably comprehensive work that competently fills a substantial gap in both the history of Richmond and the exploration of the urban experience in the United States. It rests primarily on extensive sifting of the materials in four Richmond newspapers, the city council records, the manuscript and published census returns, city directories, diaries and memoirs, a number of manuscript collections in various Virginia depositories, and a wide array of secondary works. One surprising omission is the failure to use any reports from federal officials during the Reconstruction years.

The picture that emerges is that of a city controlled by political and industrial leadership groups that were consistently conservative—rejecting annexation, reluctantly expanding city services, and refusing to employ new industrial technology—with a white population more and more given to ancestor worship. Frederick Law Olmsted characterized Richmonders in 1860 as, “a people who have . . . a disposition within themselves only to step backward” (p. 23). And Chesson, who sees a strong continuity between wartime and postwar Richmond, can only agree. Not only did the city inherit “the worst of both the Old and the New South” (p. 210), but, as he writes in his last sentence, “The tragedy of Richmond after the war was that its white leaders, after two decades of flirtation with progress, returned to a cause that they had all but abandoned and embraced the dead thing with a passion they had never felt while it lived” (p. 210). One of the strengths of *Richmond After the War* is that Chesson makes the reader share, in some measure, his feeling of emptiness and loss.

LEONARD P. CURRY
University of Louisville

BURTON W. FOLSOM, JR. *Urban Capitalists: Entrepreneurs and City Growth in Pennsylvania's Lackawanna and Lehigh Regions, 1800–1920*. (Studies in Industry and Society, number 1.) Baltimore: Johns Hopkins University Press. 1981. Pp. xiv, 191. \$16.50.

In recent years historians and other social scientists, in explaining the process of change in human affairs, have tended to subordinate the role played by individuals to broad environmental changes. In line with this tendency, students of economic growth, a process that has come to occupy a steadily more important place in economic history, have tended to downplay the functions of entrepreneurs in favor of geographical and sociological factors.

In this volume, Burton W. Folsom, Jr., has re-emphasized the role of entrepreneurs by a detailed and comprehensive study of the careers and achievements of leading businessmen in seven cities in the Lackawanna and Lehigh Valleys of northeastern Pennsylvania during the nineteenth and early twentieth centuries—Easton, Carbondale, Allentown, Wilkes-Barre, Scranton, Bethlehem, and Mauch Chunk.

In describing the rise, fall, or stagnation of business and industry in these cities, Folsom has shown that initial advantages, such as availability of raw materials, access to transportation routes, and central location within a hinterland providing a profitable market, were of much less importance than the activities of the elites who conducted the business affairs of the cities.

For all of them the initial stimulus to growth came with the burgeoning use of anthracite coal as a fuel in the 1820s and 1830s and the subsequent rise of an iron and steel industry. In the Lackawanna region, Scranton surged forward to become a leading industrial center even though Carbondale and Wilkes-Barre had better locational advantages. The main reason was the Scranton family and the industrial elite that they built up from their kinship group and immigrating businessmen. But Scranton faltered from 1880 to 1920 when the elite group fragmented and became devoted to social and political objectives apart from industrialization.

By contrast, Bethlehem, in the Lehigh region, became a major industrial center after a long static period when Charles Schwab, a “spectacular entrepreneur,” developed the Bethlehem Steel Company there.

The other cities examined by Folsom show varying degrees of industrial growth depending on the amount of drive—or lack of it—exhibited by leading businessmen.

Written from a wide range of primary and secondary sources—mostly local histories and biographies—Folsom presents an immense amount of personal detail to support his thesis. This makes the book very persuasive, but it overshadows other

factors important in economic growth such as the technological advances made by humble workbench technicians who do not figure so prominently in the historical record.

Nevertheless, this is an excellent pioneering study both in concept and execution and should serve as an inspiration for others like it.

ELISHA P. DOUGLASS
University of North Carolina,
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JUSTUS D. DOENECKE. *The Presidencies of James A. Garfield and Chester A. Arthur*. (American Presidency Series.) Lawrence: Regents Press of Kansas. 1981. Pp. xiii, 229. \$15.00.

The Presidencies of James A. Garfield and Chester A. Arthur is a volume in the "American Presidency Series," a set of works now appearing designed "to present historians and the general reading public with interesting, scholarly assessments of the various presidential administrations." Justus D. Doenecke clearly fulfills that goal in this volume, which expertly summarizes the large and excellent set of studies of late nineteenth-century politics and diplomacy published in recent years. Doenecke has a clear understanding of the main structural issues in American politics and diplomacy, few axes to grind, and presents his results in clear and concise prose.

The work is an impressive reminder of how much more we know about the Gilded Age than we did even a few years ago. This is particularly the case with the Garfield and Arthur administrations where we have the benefit of the authoritative biographies of Garfield by Allan Peskin, and Arthur by Thomas C. Reeves as well as the extremely thorough diplomatic history of this period by David Pletcher that has since been supplemented by a number of detailed studies of specific incidents.

Students familiar with this recent literature will find few, if any, surprising conclusions. Garfield, in Doenecke's account, was a weak man whom circumstances maneuvered into a position of defending the prerogatives of the president. Arthur was essentially a passive president, but a solid administrator who also contributed to the revivifying of an office that had declined during the administrations of Andrew Johnson and Ulysses S. Grant. Doenecke is—correctly I think—harsh on James G. Blaine, siding with the traditional view of Blaine as inept and opportunistic, rather than seeing him as the prophet of a "large policy." Overall, one of the main virtues of the book is its excellent survey of diplomacy under Blaine and Frelinghuysen.

Historians and readers of history are always interested in the presidency. This alone is doubtlessly sufficient justification for an "American Presidency Series." Yet, it must be noted, this is very much what one might call the "old political history," what

Thomas Cochran long ago referred to as the "presidential synthesis." Doenecke's excellent first chapter "Institutions in Transition" indicates his grasp of the new literature with its emphasis on processes rather than events (and here I refer not only to quantitatively based political and social history). Nonetheless, the remainder of the volume is essentially cabbages and kings: diplomacy, elections, appointments, political battles, Southern policy, and the like. In this sense, this volume, and perhaps the entire series—time will tell—is a step backwards from the struggle for a more adequate synthesis begun a quarter century ago by Samuel Hayes in *The Response to Industrialism*, extended by Robert Wiebe in *The Search for Order*, and continuing vigorously in the current scholarship of the Gilded Age.

ROBERT MARCUS
Rollins College

RICHARD L. MCCORMICK. *From Realignment to Reform: Political Change in New York State, 1893–1910*. Ithaca: Cornell University Press. 1981. Pp. 352. \$25.00.

This is a political history of the process by which New York State entered the Progressive era. Although anything but a neglected subject, no monograph, biography, or autobiography, however illuminating about this or that facet, reconstructs all the stages from the 1890s into the next decade. Richard L. McCormick attempts to do just that, and it is pleasant to report that *From Realignment to Reform*, which blends findings of his own with those of others, fills an important gap in the literature.

To prepare the reader for the distance that Albany was to travel by the time Charles Evans Hughes left the governor's mansion in 1910, McCormick opens with a chapter on the pre-1890s political order. Even specialists—no, particularly they—will admire the deft condensation and adaptation of previous scholarship. In less than twenty pages McCormick recreates a politics based on unregulated machines, loyal voters, governmental aid to private enterprise, and alternating rule between the two major parties.

He then goes on to trace, in succeeding chapters, the alterations of those characteristics. After realigning elections in the mid-1890s, the Republican party dominated state government for the next fifteen years. At the same time, pressures for reform built up, from outside as well as within the GOP. Eventually, they found expression in legislation that aimed, at one and the same time, to curb abuses in the machine and the economy.

The explanation for the emergence of a regulatory state does not lie, according to McCormick's evidence for New York, in one or another monistic interpretation of Marxist or social science origin. He seeks answers in the circumstances and thought and actions of the then-governing Republican party.

The resulting account is political analysis at its most convincing. McCormick examines leaders, rivalries, insurgencies, the economy, interest groups, issues, voting returns—all against a background of *events*, especially scandals discrediting political bosses and privileged businessmen. Two such scandals in 1905, McCormick recalls in a vivid chapter, prepared the ground for the election of Governor Hughes, whose administration did more than any other to enact aspirations for political and economic reform that had surfaced some ten years earlier.

It remains to say that the social justice side to the Progressive era barely shows in *From Realignment to Reform*. The fault is not the author's. A welfare (as against regulatory) state evolved in Albany under Democratic auspices in the decade after 1910, the terminal year for McCormick's study of Republican ascendancy. Stated differently, the author takes New York into but not through the Progressive era. That was his purpose, presumably, and he achieves it in a history at once thoughtful and learned.

ARTHUR MANN
University of Chicago

LAWRENCE FOSTER. *Religion and Sexuality: Three American Communal Experiments of the Nineteenth Century*. New York: Oxford University Press. 1981. Pp. xi, 363. \$19.95.

In a time of intense questioning concerning marriage, the family, and sexual roles, it is appropriate, Lawrence Foster suggests, to look for insight in the ideas and practices of those who have faced similar questions in the past. Thus, Foster's study, *Religion and Sexuality*, examines in rich detail the social institutions of nineteenth-century Mormons, Shakers, and Oneida Perfectionists.

Rejecting the image of the three groups as an "American sideshow," Foster views the communitarian enterprise as a response to the difficult social problems of antebellum America, with particular emphasis on problems relating to marriage and the family. Foster notes that although there were obvious differences in the family structures of the three groups (the Mormons practicing polygamy, the Shakers celibacy, and the Perfectionists a system of group marriage called complex marriage) the three shared certain characteristics that justify considering them together. Foster's primary interest is in questions like these: What motivated the prophet or founder of the group? What impelled individuals, presumptively normal, to join and to remain in the group? How was the transition between the old and new orders accomplished? The attempt throughout is to view the groups and their institutions as answers to particular social questions.

The narratives relating to the Shakers and the Oneida Perfectionists are interesting and well done,

but it is the treatment of the complexities of Mormon society that seems particularly commendable. Still, at least from a legal point of view, it would have been useful if the discussion of Mormon divorce had distinguished more systematically between legal divorce and marital breakdown, and it would have added a dimension to our understanding of the Mormon experiment with adoption if it had been seen in the context of the development of the modern American law of adoption.

Foster's book is based on a careful study of primary materials (manuscript and printed) and makes good use of anthropological perspectives. One of the strengths of the book is that areas where our knowledge is inadequate are specifically identified. *Religion and Sexuality* presents a very valuable account of the formation and development of the alternative social systems of three important American communal experiments.

CAROL WEISBROD
School of Law
University of Connecticut

HORACE L. FRIESS. *Felix Adler and Ethical Culture: Memories and Studies*. Edited by FANNIA WEINGARTNER. New York: Columbia University Press. 1981. Pp. xi, 272. \$22.50.

When Felix Adler returned to the United States in 1876, three years of study in Europe had confirmed his resolve to be "the minister of a new evangel." He would not preach the religion of an individual God, but would disseminate instead the message of the "Moral Law." When he spoke to the congregation of his father, a noted leader of Reformed Judaism in Germany and America, it was clear that he had reformed himself out of even the most liberal wing of the paternal faith. The logical culmination of this progression was the Ethical Culture Society, founded by Adler in 1876 in New York City and since then invariably associated with his name.

We have here a work that is both memoir and biography. Horace L. Friess was a Columbia University philosopher from 1919 to his death in 1975; he was also a son-in-law of Adler. This connection notwithstanding, Friess writes objectively and provides a well-rounded portrait of Adler's work and philosophy. With attention to Adler's family and personality, to his friends and associates in the Ethical Culture movement, and to his difficult passage between formal religion and a mere religion of humanity, this book gives us a more engaging study than Robert S. Gütchen's *Felix Adler*, published in 1974 and devoted mostly to formal analysis of Adler's thought.

Adler was no boldly innovative thinker, and Friess makes no such claims for him. Nevertheless, he represented an important strand in American cul-

ture. For despite his break with the temple, Adler reflects the heritage of Reformed Judaism in definite ways. He called himself a religious man, refuted atheism, and pressed Reformism's concern for a universal humanitarian ethic into his own moral philosophy, denoted by what Adler called "the sense of the morally infinite." Under Adler, Reformed Judaism merged with the most liberal side of American Protestantism, a fact symbolized by Adler's tenure as president of the Free Religious Association of Boston. Adler's own religious and moral ideas further confirm this alliance. Although Friess gives careful attention to these ideas, he might have noted how closely they parallel those of Josiah Royce. Each incorporated an anti-individualistic prejudice into a sense of the emerging world community; Adler's sense of that heroic struggle was only slightly less theistic than Royce's.

But Adler's zeal for the cooperative ethic and its realization in human affairs yielded certain ironies. On the social and economic issues of the day, and especially the cause of labor, Adler took a progressive stance, denounced laissez-faire capitalism, and welcomed Marx's analysis of the alienation of the working classes. The same ethic, however, gave a traditionalist cast to Adler's views on women and the family. He welcomed more education for women, but with the hope that it would make them better partners for their husbands and better mothers of their children. Toward female suffrage he was profoundly skeptical. In a world whose ethical bonds were everywhere threatened, women, like the capitalists, could not legitimately make themselves their own ends.

Adler's hope for a world community yielded a friendly posture toward some forms of "benign" imperialism toward "uncivilized" peoples. Yet Adler was so convinced that World War I had merely confirmed the power of the commercial and militarist groups in Europe that he ardently opposed Woodrow Wilson's design for the League of Nations. Here Adler succumbed to the occasional utopian strain in his thinking and called for a "parliament of parliaments" derived from democratic elections in all nations of the world.

J. DAVID HOEVELER, JR.
*University of Wisconsin,
Milwaukee*

LAWRENCE D. ORTON. *Polish Detroit and the Kolasiński Affair*. Detroit: Wayne State University Press. 1981. Pp. 229. \$19.95.

Polish immigrants in America took their Roman Catholicism and its traditional religious customs seriously. When Dominic Kolasiński came from Austrian Galicia to the fledgling Polish colony on

the east side of Detroit in 1882, most of the Polish immigrants followed this elegant and urbane priest blindly and passionately, as a link to their Polish past and a support for their Polish identity. Within the short space of three years, Kolasiński's energy and the immigrants' generosity enabled the Polish community to build a massive parish church of cathedral proportions. But Kolasiński was an ambitious and arrogant man, and when the bishop of Detroit questioned the financial management of his new parish Kolasiński defied him, refusing to hand over the parish books. For the next nine years, from late 1885 to April 1894, the Catholic church in Detroit was plunged into the "Kolasiński Affair."

Kolasiński's defiance of ecclesiastical authority split the Polish community of Detroit into two factions and resulted in a series of church closings, interdicts, and excommunications. The Kolasiński affair is a fascinating tale, which includes several riots by outraged Polish parishioners, a sensational murder trial, midnight police raids in "Polacktown," a Rasputin-like Kolasiński lieutenant, forged Vatican documents, and an ordinary priest impersonating a bishop at a crucial juncture in the affair. The story climaxes in 1894 when Kolasiński turned what was supposed to be a public recantation of his errors and apology for his sins into a triumphant vindication. Though later schismatic priests of the Polish National Catholic Church (founded in 1899 in Scranton, Pennsylvania) looked to Kolasiński as one of their forerunners, the firebrand priest and his loyal flock in Polish Detroit returned to the Roman fold.

Lawrence D. Orton has related this remarkable story clearly, crisply, and with great enthusiasm. Though his sympathies clearly lie with the Polish immigrants and their priest in this quarrel against first a German then an Irish bishop, he exercises caution in his judgments and does not cover up Kolasiński's glaring faults. Orton's book is narrowly focused on Kolasiński and the church troubles; a final chapter, presenting an overview of the Detroit Polish community in the late nineteenth century, is poorly integrated with the rest of the book. Nonetheless, this work relates a significant chapter in both Polish-American and Catholic church history. Obviously, the general public in Detroit followed the Polish church troubles avidly, for Orton has written his account largely from the English language press of the time, which aired the affair thoroughly. It may well be that the Kolasiński affair is partly responsible for the unfavorable public image that Poles have experienced in Detroit to this day.

All in all, Orton's work is a slender but worthwhile volume.

EDWARD R. KANTOWICZ
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Ottawa*

PATRICK J. GALLO. *Old Bread, New Wine: A Portrait of the Italian-Americans*. Chicago: Nelson-Hall. 1981. Pp. xi, 356. \$16.95.

Old Bread, New Wine expresses the hurt and anger that many Italian-Americans have felt when they reflected on outrages experienced by their parents' generation in America or when they directly encountered discrimination and affronts because of their ethnic heritage. The book spans the Italian experience in America beginning with Columbus and ending with the Italian-American heroes of "Watergate" (Sirica and Rodino) and the urban social issues of the 1980s. Patrick J. Gallo affirms the central tenet of the "new ethnicity" movement that group identity and roots are vitally important to personal identity, character, and psychological well-being. In keeping with his concern for consciousness-raising he adopts an unremitting "we-they" perspective in which "they" are the vaguely specified WASP establishment, though in the case of religion "they" are Irish Catholics.

The book is a highly personal account of Italian-American history written from a select group of secondary sources and directed toward the general Italian-American population rather than specialists in immigration and ethnic history. Its usefulness in the classroom is limited by Gallo's commitment to Italian ethnicity, which mutes his sensitivity to class influences and causes him to forego opportunities for comparative history that would provide perspective from the experiences of other immigrant and native groups.

Old Bread, New Wine is, to borrow the author's words, "a portrait of courage, hard work, love, and humanity" intended to be "a source of self-awareness, pride, and strength for future generations."

JOHN W. BRIGGS
Syracuse University

LEO PAP. *The Portuguese-Americans*. (The Immigrant Heritage of America.) Boston: Twayne. 1981. Pp. 300.

Leo Pap's *The Portuguese-Americans* is both a useful and in some ways disappointing book. The author begins with the earliest Portuguese contacts with the United States and traces the first Portuguese settlements to the most recent (post-1965) immigrants. In addition to the story of migration, he discusses important aspects of immigrant life and Portuguese-Americans—their economic conditions, religion, institutions, such as newspapers and civic groups, and cultural traditions and their survival in the New World.

The book reflects many of the older rather than newer trends in social and immigration history; for this reason, it might disappoint many scholars inter-

ested in social mobility, assimilation, acculturation, and family life of these immigrants and subsequent generations. Pap does discuss these topics and includes data, but without much awareness of the newer approaches to these important issues. Hence one will find little quantitative information on intergenerational mobility or changing family customs. For instance, he discusses the immigrant community's occupational and economic life and jumps to the present Portuguese-American community with little consideration of the intervening years. The descriptions and details could stand more analysis.

At times Pap simply lists or catalogues various aspects of Portuguese-American life, in part to illustrate points about these immigrants. This style is not the best. One listing shows the "contribution of the Portuguese element to the United States" (p. 233). This "contribution" approach appears a bit dated and nearly defensive, which is hardly necessary. The author seems eager to include nearly all topics, which means that some broad themes are left undeveloped.

The strength of the book lies in descriptions of immigrant communities in Massachusetts, Rhode Island, California, and Hawaii and immigrant life in these communities. Pap also includes some rather interesting comparisons about these different communities. These chapters are informative, carefully researched, and often detailed. In addition, the author includes a bibliography that is valuable and extensive.

Anyone wishing to know about Portuguese-Americans, especially the immigrant generation, will have to begin with Pap's account. Because so little has been written about Portuguese-Americans and because his book is a comprehensive survey, it will quickly become the standard work on the subject. Students of immigration history are indebted to Pap for an informative, if limited, book on Portuguese-Americans.

DAVID M. REIMERS
New York University

JOAN MARK. *Four Anthropologists: An American Science in Its Early Years*. New York: Science History Publications. 1980. Pp. 209. \$20.00.

This lucid, unpretentious volume is worth the attention of intellectual historians and historians of the human sciences for two reasons. It offers a group of interlocking biographical and institutional studies in the generation of native-born American anthropologists active between 1870 and 1910 who named and popularized the subject, founded organizations, departments, and publications, and established traditions of fieldwork. It is also an essay in nationalistic revisionism, an effort to redress the balance of historical justice toward Frederic W. Putnam, Alice

W. Fletcher, Frank H. Cushing, and William H. Holmes, who were dismissed as prescientific amateurs or ignored entirely by Franz Boas and his students when they came to dominate the historical memory as well as the theoretical and research interests of the profession. This more controversial, and stimulating, aspect of the book suggests (though it does not treat at length) a number of questions about professionalization, group loyalty and hostility within professions, and the conflict between native and immigrant scholars in encouraging the succession of scholarly paradigms.

On the sober empirical level, Joan Mark offers useful detail on the process by which these pioneers discovered their interest and carved out institutional careers—often by creating the institutions—that allowed them to make vocations from avocations. The book is richest on scholarly politics in the broad sense—personalities, the wooing of patrons, institutional competition, and so forth. While there are scattered treatments of theory and method, these are usually not sufficiently detailed and comparative to establish a case for these founders as intellectual influences worthy of comparison with Boas and his students. A partial exception is the evidence presented in chapter 4 that the eccentric, empathetic Frank Cushing adumbrated a recognizable version of cultural pluralist theory almost two generations before Ruth Benedict popularized a more sophisticated psychological version of it in the 1930s.

The student of academic watersheds or paradigm change will find much suggestive material here. In an epilogue Mark notes two major reasons for the submergence of her heroes—their lack of formal scholarly credentials and the growing desire, fostered but apparently not created by Boas, for clear lines of authority in which the direction of research and certification of theory would be concentrated in as few centers as possible. Another reason emerges clearly from her data—the remarkable mixture of industry, charisma, and resentful paranoia that constituted the personality of Franz Boas. Boas could brook no rivals and was convinced that his own theories and methods at any given moment exhausted the realm of respectable science.

Beyond these clear factors, the account here suggests comparison with the need of so many intellectual generations since Emerson's to sweep the board clean and start afresh and to remove the filters of inherited authority and view the external world clearly. This was easy enough for Emerson, but professional scholars, officially dedicated to thoroughness and respect for past achievement, need to find good reasons for rejecting the disciplinary heritage. Hence the drives for stiffer credentials, tighter methods, and allegedly more neutral and comprehensive interpretations. Both Mark's book and George Stocking's invaluable *Race, Culture*

and *Evolution* (1968) suggest as well the importance of cultural politics—the European intellectual's sense of superiority to provincial scholars and resentment of nativism, later the attraction of anthropology for alienated native scholars.

Such ritual slaying of elders can also energize work in the history of scholarship. Certainly Mark's desire to do justice to these forgotten founders is associated with contemporary polemics, the distaste for "positive science" in the sense of a system of universal categories through which all human phenomena can be classified and explained. Her admiration for Fletcher and Cushing stems in part from a view of them as predecessors of Ruth Benedict rather than someone like Talcott Parsons. This attempt to rediscover ancestors for a more pluralist approach to theory and to the distribution of scholarly authority is legitimate and humane, though it suggests serious questions about the limits of tolerance in scholarship and the existence of other criteria for measuring the value of contributions to knowledge. The book is of value both for its data and for suggesting such questions.

FRED MATTHEWS
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Toronto

DAVID F. TRASK. *The War with Spain in 1898*. (The Macmillan Wars of the United States.) New York: Macmillan. 1981. Pp. xiv, 654. \$29.95.

In 486 large and crowded pages of text and 137 of notes, David F. Trask offers an examination of the Spanish-American War that is both massive and magisterial. It is a political-military history whose judgments will work further to rehabilitate the reputation of William McKinley and to diminish the influence of imperialistic motives in the formulation of American policy.

In Trask's rendering, McKinley, though yielding to public clamor for a war he did not want, became a war leader of "sound strategic insight" (p. 484) and a forcefulness sufficient to hold often wavering military commanders to his central conception, that of ending the war as quickly and inexpensively as possible by attacking the peripheries of the Spanish empire until cumulative pressures compelled Madrid to sue for peace. At the outset Cuban independence was his sole war aim. In the wake, however, of expeditions to the Philippines born alone of this desire to destroy Spanish resistance, Washington moved to acquire Guam and Hawaii to strengthen Pacific communications routes and, by mid-summer, to retain Puerto Rico and a Philippine port. Finally, another popular outburst compelled McKinley, personally opposed to acquisition of the Philippines, to demand of Spain the entire archipel-

ago. Empire was "neither planned nor even anticipated before the brief conflict with Spain" (p. 483).

The book's virtues are many: the author's often persuasive judgments; the scrupulous care with which he treats sources; the illuminating integration of American, Spanish, Cuban, and Filipino perspectives. There is a tediousness in so many detailed exchanges, but it more often reflects the pettiness of Alger, Miles, Shafter, Sampson, Dewey, and Schley than any deficiency in the author's descriptive skills. (Only McKinley and Secretary of the Navy John Long rise above the bickerings of self-service.) This is, in sum, a work that will long remain the major reference volume on the war of 1898.

David Trask's book will not, however, exhaust the historian's agenda. He offers a convincing case that strategic, economic, and ideological considerations were less important contributors to war than 1898's "ungovernable burst of popular emotion" (p. 474), and he twice asks the vital question, how are we to account for that ebullition? But to his answer he devotes scarcely one and a half of his 486 pages, and we are left to gnaw such phrases as "irrational impulses" (p. 59) and "pent-up emotions . . . building . . . among the American people for a generation" (p. 475). Granted he stipulates that he means to treat the conduct of the war rather than its causes or consequences, yet causes sway conduct more than he grants. Although he acknowledges two results of the public's intrusions into policy—one initiating war, the other compelling retention of the Philippines—in fact popular agitation remained high throughout the war, expressing itself in obstreperous demands for places in the military and for immediate combat, in coastal panics, in eruptions of excessive fright and excessive jubilation, in near-riotous training-camp disorders; many such factors invaded McKinley's policy calculations. Emotion permeated the war's origins and execution, and thus vital aspects will remain shrouded until historians are able to give greater analytical substance to such forces. There are in the author's arresting reference to Americans' "unfathomable, immeasurable, and paradoxical combination of confidence and insecurity" (p. 475) only the beginnings of exploration. It is as if the book invites us to watch the passage of a long file of railroad cars, each of which Trask has painted in illuminating color, while the train's engine remains invisible.

GERALD F. LINDERMAN
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Ann Arbor

DOMINICK CAVALLO. *Muscles and Morals: Organized Playgrounds and Urban Reform, 1880–1920*. Philadelphia: University of Pennsylvania Press. 1981. Pp. xiv, 188. \$21.50.

In the preface, Dominick Cavallo warns readers that this book is not about sport, recreation, or team games. Its "only subject" is "the movement to organize children's play, and its cultural and political implications" (p. xi). *Muscles and Morals* has a narrower focus: the origins and ideology of the Playground Association of America and the intellectual debts of its founders to contemporary psychologists, educators, and reformers. Cavallo handles these themes with skill, and his monograph examines an interesting corner of Progressive era thought.

In the late nineteenth century, playgrounds, vacation schools, and small parks were established as alternatives to the physically and morally dangerous street play of slum children. But the absence of standards and ideological consistency among playground supervisors troubled Henry Curtis, who had studied with G. Stanley Hall, and Luther Gulick, Jr., a pioneer in the physical education and school playground movement. With the help of Jacob Riis, Jane Addams, other settlement house workers, and Boston philanthropist Joseph Lee, Gulick and Curtis started the Playground Association of America in 1906. Using this forum, they spread the word that proper playground supervision of male adolescents could develop analytical powers and habits of cooperation, promote Americanization, reduce ethnic conflict, and produce "group-directed adults" who would make "city life less frightening and more humane" (p. 103). The play organizers were influenced by the theories of G. Stanley Hall, John Dewey, and others, but they originated the idea that supervised team games could instill in players some worthwhile feminine traits, especially moral intuition and empathy, without destroying masculine competitiveness and ambition. This "integration of stereotypical sexual-social characteristics," says the author, "was the heart of the play movement's ideology" (p. 10). It also explains his decision to devote the final chapter to Jane Addams's youthful wavering between male and female role models.

Readers interested in the cultural and political implications of the playground movement will have no difficulty suggesting more pertinent themes than Addams's adolescent adjustment. What political forces were behind the dramatic increase in the number of playgrounds from fewer than one hundred in 1905 to nearly four thousand in 1917? Where in the city were the playgrounds located, and did clientele shift from slum to middle class? Was the PAA responsible for the new playgrounds, or were they created for unsupervised sandlot baseball players as Gunther Barth suggests in *City People*? Why was poor attendance a problem on supervised playgrounds, and what did school athletic directors say about the play organizers? Cavallo recognizes the potential for conflict within the PAA, but instead of exploring it he falls back on Kai T.

Erickson's "perceptive insights into the nature of culture" (p. 153). He does not attempt to measure the effectiveness of playground social training because the users' reaction "is dust in the wind" (p. 155).

The title and preface promise more than this book delivers. Yet *Muscles and Morals*, a study of the ideology of organized play, does make a contribution to our understanding of Progressive reform.

LOUISE C. WADE
University of Oregon

SARAH STAGE. *Female Complaints: Lydia Pinkham and the Business of Women's Medicine*. New York: W. W. Norton. 1979. Pp. 304. \$14.95.

For well over a century Americans have dosed themselves with native proprietary medicines, yet few historians have investigated the growth, development, and marketing techniques of individual proprietary medical companies. In *Female Complaints: Lydia Pinkham and the Business of Women's Medicine*, Sarah Stage helps to fill this gap with a lively, well-documented history of the company that produced perhaps the industry's best-known remedy: Lydia E. Pinkham's Vegetable Compound.

Nineteenth-century medical thought generally saw women as biology's victims, governed by delicate, irritable, and interconnected reproductive and nervous systems. Charged with supervising family health, yet frequently ill or convalescent themselves, women sought relief from uncertain sources: orthodox "heroic" practitioners and theoretic nihilists; Thomsonian, homeopathic, and hydropathic sectarians; domestic healers, and quacks. Citing the age's distrust of regular medical therapeutics, Stage notes the connection between women's preference for proprietary medicines and their earlier roles in domestic and botanic practice. Thus, in 1875 Lydia Pinkham, a social reformer tied to abolitionism and temperance, began to market her alcohol-based botanic remedy "in the belief that it provided women with a safe alternative to the therapies employed by medical doctors" (p. 88).

Initially the Pinkhams touted the Vegetable Compound as a "cure" for various ailments, including "weakness" of the reproductive system, kidney complaints, uterine prolapse, menstrual dysfunction, and labor pains. Long after her death in 1883, personalized Pinkham advertising featured Lydia's portrait with invitations to write to her for free medical advice. James T. Wetherald, the "architect of Pinkham ad policy" (p. 191), employed sensational saturation advertising and for forty years appropriated contemporary medical thought to expand his market. Stage shows how he used the rhetoric of perfectionism to convince women that invalidism was unattractive and that ill-health stemmed from

ignorance at the same time as he capitalized on Victorian prudery by insisting that regular use of the compound brought the benefits of science while eliminating the need to consult male physicians and surgeons.

In the twentieth century the company's phenomenal success was undermined by internal and external forces: feuding between the Pinkham and Gove family branches, and attacks from muckrakers, the American Medical Association, and government regulatory agencies. Making effective use of the Pinkham collection at the Schlesinger Library, the author carefully details the effects of the internecine struggle. Her thorough examination of advertising copy and company records reveals the fascinating nuances of Pinkham attempts to circumvent regulation by reworking formulas, modifying labels, and substituting testimonials for unsubstantiated claims. She guides the reader skillfully through the intricate maze of regulatory enforcement problems and clarifies the role of voluntary compliance played by the Proprietary Association.

Stage has set the Pinkham experience against the background of changing medical and cultural attitudes toward women over a century. Her thoughtful, provocative analysis of Pinkham literature and marketing tactics is at its best for the decades between 1890 and 1930. The discussion of women and earlier nineteenth-century medicine, while lucid and well written, is based largely on secondary sources and adds little that is new. Occasionally the author's ambitious attempt to combine biography, economic, social, and medical history leads to a blurring of focus. These minor criticisms, however, are not meant to detract from Stage's informed and useful study.

JANE B. DONEGAN
Onondaga Community College

NANCY SCHROM DYE. *As Equals and As Sisters: Feminism, the Labor Movement, and the Women's Trade Union League of New York*. Columbia: University of Missouri Press. 1980. Pp. 200. \$17.50.

Although there have been several studies in the past few years dealing with working women, this is the first in nearly forty years to treat specifically the Women's Trade Union League (WTUL). A revised dissertation, this thoroughly documented monograph is based upon manuscript collections of the League and its leaders, interviews, and a wide variety of published material including public documents and League literature. It contains perceptive biographical vignettes of prominent League members and an interesting composite of its upper-class adherents or "allies."

Concentrating on the period from its establishment in 1903 to World War I, Nancy Schrom Dye

analyzes the New York League whose purpose was to improve female workers' conditions and their status in the labor movement. She contends that by its unique insistence on an egalitarian cross-class alliance between working women and philanthropists the organization embodied the principles of early twentieth-century feminism. Since belief in sisterhood was not always compatible with belief in class solidarity, members frequently were in a dilemma. Support for protective laws and separate unions opened them to the charge of dividing the working class; however, if they stayed away from women's issues they were guilty of ignoring the special problems of female workers. For the first few years the League concentrated on unionization, but, disillusioned by the hostility of organized labor, especially the rank and file, it began instead to mediate women's needs with unions. By 1914 the New York WTUL relied primarily on suffrage extension and legislation to protect women workers. World War I completed the transformation from labor organization to social reform, as the conviction that women composed an oppressed group within the labor force replaced the original belief that women's problems in the workplace were inseparable from those of men. Sex not class had become the primary distinction for the New York League. But, according to Dye, as the task of unionizing women was left to uninterested men the League's choice of suffrage was a retreat, not a feminist victory.

Unlike some scholars, Dye asserts this shift was not a result of upper-class domination, for although the organization was characterized by class conflict there were no sharp divisions along class lines. There were many cohesive factors resulting in a feminist network so strong that working-class women often were more comfortable with their upper-class sisters than with male workers.

Especially timely because of the selection of Joyce Miller to the AFL-CIO Executive Council and the decision of the Coalition of Labor Union Women, like the League, to admit nonunionists, Dye's study could have been more valuable if she had broadened her focus. Her almost cavalier dismissal of the League's organizing efforts after World War I and her failure to examine other League branches, the national organization, or even relate the New York experience more directly to these groups is disappointing. As much as one might desire a more complete account, her study is, nevertheless, a valuable synthesis of feminist and labor history.

JAMES KENNEALLY
Stonehill College

JUDITH ICKE ANDERSON. *William Howard Taft: An Intimate History*. New York: W. W. Norton. 1981. Pp. 277. \$17.95.

By "intimate," Judith Icke Anderson may mean "characterizing one's deepest nature." If this is so, to plumb Taft's "deepest nature" she uses a psychohistorical approach based largely upon the ramifications of his obesity, "a condition from which Taft quite palpably suffered through most of his life" and that affected his personality and career (p. 292). Because he was huge—up to 355 pounds—"Fatso" was often the butt of jokes about his inability to tie his shoe laces and need for extra-large clothes, bathtubs, and horse saddles. More important, by impeding his breathing, his obesity caused him to fall asleep easily, especially soon after eating. It also added to his tendency to be lethargic, to procrastinate in making decisions, to put off doing what he must do until the last possible moment, and then, under pressure, eating all the more. A conclusion reached is that Taft lost weight when he was happy, gained weight when worried.

Another definition of "intimate" is that of "warmth" and "closeness." As Margaret Leech in *The Days of McKinley* does for McKinley and Ida, so Anderson provides illuminating insights into Taft's relations with his wife, Helen (Nellie) Herron Taft, and also with Theodore Roosevelt. Her thesis is that Taft depended heavily upon others for leadership, first upon his parents, then his wife until she became ill in 1910, and upon Roosevelt until they split. As various other Taft and Roosevelt biographers have related, Nellie and Roosevelt in particular pushed Taft from a judicial to a political career, one for which his simple and generous nature was ill-equipped. Unlike Roosevelt, he took criticism of his work to be personal; disliking politics, he tried to please all parties and persons and ended up vacillating; given his conservatism, deification of the Constitution, and reluctance to use the power of the presidential office, he could not be a Progressive in a Progressive era even if the host of reforms passed during his term regularized and added to those of Roosevelt. To escape from Nellie and his "persecutors" in Washington, he took long train tours while president, eating what he pleased, or played golf or engaged in social diversions that increased the volume of his unfinished business and often denied him the time to prepare speeches lacking "gaffs."

Although Anderson frequently uses apt quotations from or about Taft that she found in a galaxy of primary and secondary sources, she cites no sources for them. Her narration of Taft's experiences in the Philippines, as secretary of war, as president, and as chief justice is best when dealing with judicial or domestic issues; it is poor on foreign and military affairs, which she barely mentions. For those familiar with extant biographies of Taft, Anderson adds keen psychological insights into both Taft and Nellie. For greater detail on both Taft's

presidential and judicial career, one must still turn to other biographers, especially to Henry F. Pringle and Alpheus T. Mason, and for foreign affairs to Walter and Marie Scholes, George E. Mowry, and, modestly, yours truly.

PAOLO E. COLETTA
U.S. Naval Academy

ROBERT E. FICKEN. *Lumber and Politics: The Career of Mark E. Reed*. Santa Cruz, Calif.: Forest History Society or University of Washington Press, Seattle. 1979. Pp. xi, 264. \$14.95.

Until the appearance of Robert E. Ficken's well-written and deeply researched biography, Mark Reed was little known even to specialists in the history of the Pacific Northwest. Reed is significant because of his leadership in the region's timber industry during the first third of the twentieth century and his career in politics during the era of Republican ascendancy from the election of Warren Harding to the advent of the New Deal.

Mark Reed was decidedly a late bloomer in his impact on political and economic affairs. As Ficken puts it, Reed's early years were marked by "considerable floundering" (p. 40). Only one of seven chapters is devoted to the first fifty-two years of Reed's life; at that age, "the most important achievements of his career, both in the lumber industry and in politics, were still ahead" (p. 41).

Although he was active in the presidential campaigns of 1924-32, and in the last months of his life headed a lobbying effort to protect timber products in the Smoot-Hawley tariff bill (a fight that was mostly unsuccessful), Reed was primarily interested in state politics. During sixteen years in the Washington House of Representatives, Reed worked with other Republican businessmen-legislators to implement the philosophy that the business of the country was business and that the interests of business were the interests of all. Reed, however, was more perceptive than most in his ability to avoid knee-jerk reactions to demands or perceived threats from radicals, labor unions, or minorities that questioned the status quo. Reed, for example, pushed successfully for medical insurance legislation and for a workman's compensation act. Ficken argues persuasively that Reed's politics "pursued private interest while stressing the interest of the community and often manag[ed] to serve both" (p. 131).

Reed is a good example of an enlightened business leader of the 1920s, but it is possible that Ficken exaggerates his importance. Aside from a brief period in the early 1920s when he wielded considerable influence with the legislature and the weak Governor Lewis Hart, Reed's accomplishments were not particularly notable. Most of the years from 1924 on were spent feuding with fellow lumberman

and two-term governor, Roland Hartley. The two men appeared to have much in common but, in fact, offered a study in contrasts: Reed, subdued, gentlemanly, thoughtful, conscientious; Hartley, crude, boisterous, swaggering, deceitful. That Hartley usually bested Reed is, Ficken notes, a sad comment on the viability of Reed's politics of "issues and reason" when confronted by Hartley's politics of "mass persuasion . . . no matter how gross or misrepresentative of the truth . . ." (p. 131). A major theme in Reed's political career was the regret that he had not run for governor in 1924 when he was virtually assured of victory. The opportunity never came again.

There is no doubt that Reed played a significant role in the timber industry as the head of Simpson Logging Company headquartered in Shelton. As in politics, Reed was noted for his ability to view the broad picture and to shape policies that benefited his company while serving the interests of the industry. The most critical and persistent problem was overproduction. Reed worked long and hard, and ultimately unsuccessfully, to persuade his associates to limit production voluntarily. After experience proved that long stretches of bad weather were the best way to curtail logging, Reed, in typically laconic but clear prose, observed that it was, "rather a discrediting admission on the part of those engaged in the industry that we must depend upon an active God to save us from the result of our own folly" (p. 148).

Reed rationalized Simpson's operations horizontally and vertically and attempted to expand his markets, particularly by sales to Japan. He was more unusual in his efforts to provide decent camps and a living wage for his loggers. The major problem with assessing Reed as a businessman is the need to know more precisely how he accomplished his goals. For example, it is stated that Simpson was more profitable than other timber companies, but we are not given figures or persuasive documentation, perhaps because they are not available.

This exemplary public biography would be welcome under any circumstances, but particularly so because of the paucity of similar works on Washington state figures, either politicians or businessmen. Only two of the state's governors (including the territorial period) have biographies. For the business community, aside from men like Frederick Weyerhaeuser and James J. Hill who were important to the state, but not strictly speaking of it, the only comparable work is that on Thomas Burke. About as scarce are books on the lumber industry (with the exception of the radical labor movement) and twentieth-century state politics. This small volume does much to help fill a number of gaps.

KENT D. RICHARDS
Central Washington University

LEONARD A. CARLSON. *Indians, Bureaucrats, and Land: The Dawes Act and the Decline of Indian Farming*. (Contributions in Economics and Economic History, number 36.) Westport, Conn.: Greenwood Press. 1981. Pp. xii, 219. \$29.95.

This is a slightly revised version of Leonard A. Carlson's doctoral dissertation in economics at Stanford University (1977), which was entitled "The Dawes Act and the Decline of Indian Farming." Its central thesis is that Indians were making slow but sure progress "toward the goal of self-support through subsistence farming before allotment" and that the imposition of allotment and subsequent regulations created disincentives that led to a decline of Indian agriculture. As an economist, the author has sought to extend the methods of cliometrics to his subject.

Historians are not likely to discover anything new or novel in the book's conclusions, but they may find helpful the author's collection of secondary sources and published statistical information. The most positive thing that can be said about the book is that Carlson has carefully searched the available secondary literature on allotment and has skillfully assembled his findings to provide the most comprehensive treatment available of allotment policy on a broad scale. This is something that no historian has attempted to do. Economists may perceive some theoretical contributions that have eluded me. I found the construction of models to explain how and when allotment was applied unconvincing, especially when the models took no cognizance of the politics of statehood or the power that statehood gave Western representatives, especially senators, in the implementation of the allotment policy.

Carlson argues that "the question of why the allotment of Indian lands . . . was drawn out to span nearly five decades has generally been neglected by historians." To answer this question he constructs two models: (1) the guardianship model, which assumes that the federal government sought to protect Indian property through allotment and (2) the demand-for-allotment model, which assumes that allotment resulted from the "potential benefits to non-Indian settlers, speculators, and merchants." After sifting large quantities of statistical material, he concludes that allotment developed "in response to the pressure of market forces" and that the guardianship model will not suffice as an explanation. His analysis makes no attempt to explain why the reservations of New Mexico and Arizona, by and large, were exempted from allotment.

Carlson's evidence for the decline of Indian agriculture after the imposition of allotment is impressive, but it is based mainly upon published statistical data taken from the annual reports of the commissioner of Indian affairs and from census reports. My

experience with these figures has convinced me that they are of dubious value, especially when categories are compared over a period of years. Carlson is aware of the problems involved but concludes that biases and inaccuracies in reporting offset one another. The "counterfactual alternative to allotment" that he offers in his final chapter is little more than a restatement of the recommendations of the 1928 Meriam Report and the economic program of the Indian New Deal. Comanche and Arikara are misspelled throughout.

LAWRENCE C. KELLY
North Texas State University

PHILIP TAFT. *Organizing Dixie: Alabama Workers in the Industrial Era*. Revised and edited by GARY M. FINK. (Contributions in Labor History, number 9.) Westport, Conn.: Greenwood Press. 1981. Pp. xxv, 228. \$35.00.

Although published posthumously as well as revised and edited by Gary M. Fink, this book bears all the distinguishing characteristics of Philip Taft's approach to labor history. It is meticulously researched (and documented), encyclopedic in content, filled with long, direct quotations from sources, and written in simple narrative style. One would have to second Fink's introductory assertion that he made a "conscious effort to retain and . . . maintain Taft's mode of analysis" (p. xxiv). Unfortunately, that editorial decision may also be the book's gravest flaw, for it exhibits a minimum of analysis.

What we do find in *Organizing Dixie*, however, is a complete skeletal overview of the institutional history of organized labor in Alabama. As befits a state in which coal, iron, and steel were the bulwarks of its industrial sector, Taft stresses the history of unionism among coal miners and iron and steel workers. Here we learn much about the experiences of the United Mine Workers of America and the Steel Workers Organizing Committee-United Steelworkers of America in the Deep South. Yet Taft does not neglect the history of other Alabama workers. Building tradesmen, railroad employees, textile millhands, and Gadsden's rubber workers all appear in the pages of this book. Separate chapters, moreover, trace the institutional history of the State Federation of Labor, the State Industrial Union Council, and the Alabama Labor Council, AFL-CIO. Naturally, almost two-thirds of the text concentrates on the period from the New Deal through the Great Society, the core era of organized labor's growth and influence in Alabama.

Here, then, we have the most complete and modern history of a state labor movement in the South written from the perspective of its institutional leaders. What we miss is any clear sense of how Alabama's labor history compares to the national

experience or precisely how Southern traditions shaped that history. Early on Taft writes that "the rural character of the industrial labor force . . . significantly influenced efforts to organize labor for collective action" (p. 6). But, thereafter, he seldom develops that theme. Nor does he explain why coal miners, steel workers, and rubber workers eventually built stable unions while textile hands persistently failed to do so. Taft also teaches us much about black-white relations among Alabama workers and especially about the progressive role organized labor played in that arena. Yet here, too, he presents the story of race relations from the official leadership perspective. In the movement for equal rights we nearly always see union officials in advance of their rank and file. We do not see William Mitch, a UMW leader of socialist background from Indiana who later became Alabama district director for the UMW, then CIO, and finally AFL-CIO director, acting as a defender of Birmingham's "Bull" Connor during the tumultuous decade of the civil rights crusade.

This might have been a far better book if Taft had heeded the advice offered by one of his teachers, the founder of labor history as an academic discipline, John R. Commons. "Keep your interpretation as close to the plane of empirical events as you can," Commons told his students, "but never give up searching for the most comprehensive kind of interpretation." Nevertheless we must still be grateful for all Philip Taft's assiduous empirical research, which has eased the task of the interpreters of labor history, and mourn the fact that we can expect no more works such as *Organizing Dixie*.

MELVYN DUBOFSKY
State University of New York,
Binghamton

LEWIS A. ERENBURG. *Steppin' Out: New York Nightlife and the Transformation of American Culture, 1890-1930*. (Contributions in American Studies, number 50.) Westport, Conn.: Greenwood Press. 1981. Pp. xix, 291. \$23.95.

The shift from the genteel middle-class cultural order of the nineteenth century to the mass consumer culture of the twentieth has attracted increased attention from American historians in recent years. Seeking to comprehend the significance of this cultural transformation, they have analyzed the various forms of commercial recreation that swiftly achieved prominence at the turn of the century, including amusement parks, vaudeville and movies, ragtime and jazz, baseball and other professional sports. In the light of such studies, the 1920s no longer appear to mark a sharp departure in American social and cultural history but instead a

culmination of developments extending back to the 1890s.

Lewis A. Erenberg's *Steppin' Out* is a further contribution to this effort. Focusing upon New York's cabarets, he attempts to examine the changing character of public nightlife and its meaning for entertainers and patrons. His discussion ranges over a variety of colorful materials: the evolution of the cabarets from roof gardens, theaters, "lobster palaces," and other restaurants; the cabaret as a battleground between moral custodians and "pleasure-mad women"; the informal setting of the cabaret and its breakdown of social barriers; Irene and Vernon Castle and the prewar "dance craze"; and the tensions of the roles of cabaret entertainers, from headliners like Sophie Tucker to the interchangeable chorus girls.

Such topics demand a keen critical balance to avoid falling either into the frothy narrative of the popular picturebook or the oozing claims of exaggerated significance. For the most part Erenberg keeps his balance, neatly synthesizing the work of previous scholars and extending it in fresh topics and analyses. Occasionally, however, he slips. He plunges into the froth, for example, in his discussion of Sophie Tucker and Texas Guinan as "friendly" entertainers, failing to probe sufficiently what lay behind this model of "friendliness" and how it flattered the customers. He slips in the other direction in making too bald and unqualified claims for his material and in a lack of clarity about his argument. "In describing the cabaret," he declares, "we are in fact analyzing the transformation in the image and reality of male and female character and the Protestant approach to passion in public life" (p. xiii). Necessarily, in studying cabaret life, Erenberg can tell us more about the image than the reality of character and passion, and the images themselves are often ambiguous. At times Erenberg appears too accepting of the cabaret's daring, "bohemian" character, its vaunted overthrow of Victorian restraints in the public display of private impulses. At other moments he associates himself with the skeptical pronouncement of George Jean Nathan: "It is all as gay as a pint of uncorked domestic champagne, as unforced as a pawnbroker's smile, as devilish as deviled ham" (p. 140). While cabarets certainly struck different notes in their various entertainments, the conviction emerges from Erenberg's narrative that they succeeded best when they fused the daring with the domesticated, flattering their patrons' sense of rebellion while recognizing their basic conventionality. Despite their devil-may-care aura, the effect of cabarets and similar recreations was to perpetuate the bourgeois order with all its structural inequities and to tighten the integration of work and leisure in a continuous cycle of getting and spending. Erenberg himself briefly voices such

a view, although he does not develop it sufficiently. In this light what is especially puzzling is the uncritical applause that breaks out periodically in his narrative and with which he ends the book, heralding the new emphasis upon consumer recreations as marking a "revitalization of American culture" and "a revitalized self" (p. 238). Nonetheless, his book is a welcome addition to the growing scholarly literature on American popular culture.

JOHN F. KASSON
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DAVID LEVERING LEWIS. *When Harlem Was in Vogue*. New York: Alfred A. Knopf. 1981. Pp. xiv, 381. \$17.95.

If one were to supply a subtitle for David Levering Lewis's kaleidoscopic study, *When Harlem Was in Vogue*, "High Culture in Black Harlem, 1919–1935," would be as appropriate as any. Lewis focuses upon a relatively familiar cast of writers and intellectuals but does so in such rich detail, gleaned from publications, interviews, and archives, that even the most knowledgeable students of the period will learn much. Moreover, their learning will be accomplished with great pleasure, for Lewis is a highly skilled stylist whose writing is always engaging and capable of transmitting the special aura of the time. He is especially good at capturing the prevailing spirit of exhilaration and correcting the late Gilbert Osofsky's thesis that Harlem in the 1920s was a tragic slum.

Lewis presents us with a series of memorable word pictures and pithy critiques: "Claude McKay was the purest of that special breed that strives for success only to find it intolerable" (p. 53). "Impotence and death run through [Countee Cullen's] poetry like dark threads, entangling his most affirmative lines" (p. 76). Alain Locke was "Eurocentric to the tip of his cane" (p. 117). Jesse Fauset's characters "have the straitened pretensions of aristocrats after a revolution" (p. 124). A'Lelia Walker "received most of the Harlem artists and sent them congratulatory notes, but spent the Renaissance playing bridge" (p. 166). W. E. B. Du Bois "invariably preferred Fauset over McKay because of the neighborhoods in which her novels lived" (p. 274).

Unfortunately, these staccato interpretations are not drawn together in any overarching framework. Lewis's focus is more on the creators than on their creation, more on narrative than on analysis. After promising chapters on the origins of the Harlem Renaissance and valuable biographies of its black promoters and white patrons, Lewis's purpose becomes blurred in a blizzard of details that indiscriminately mixes together book publishing, sexual proclivities, the production of plays, wild parties,

poetry, and gossip, and his book becomes as much a study of high society as of high culture.

Lewis seems aware that there were other Harlems besides the one he concentrates upon. He acknowledges the importance of jazz and blues, of the sporting life, and of religion, but subjects none of it to close scrutiny. Always he maintains an eye for the exotic, and it is not uncharacteristic that he devotes more space to the various sects of black Jews in Harlem than to the major Protestant churches or Pentecostal sects that were so crucial to the social and cultural life of black New York.

This endlessly fascinating account seems, ultimately, less than the sum of its parts and would have profited greatly from more overall direction, more thematic glue binding its bits and pieces together. Lacking this coherence, Lewis's considerable intellectual and literary abilities are spent on a book that is neither as satisfying nor as valuable as it could have been.

LAWRENCE W. LEVINE
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Berkeley

VICTOR GONDOS, JR. *J. Franklin Jameson and the Birth of the National Archives, 1906–1926*. Foreword by JAMES B. RHOADS. Philadelphia: University of Pennsylvania Press. 1981. Pp. xv, 232.

What cause might forge a coalition among the American Historical Association, the American Legion, the Hearst newspaper chain, and President Calvin Coolidge? The answer: a movement for the construction of a national archives. J. Franklin Jameson, during his years as director of the Bureau of Historical Research at the Carnegie Institution, was the single most important individual keeping the cause before Congress and successive presidents from Theodore Roosevelt to Calvin Coolidge. The years of legislative footdragging and bureaucratic inertia could not be overcome by Jameson's coalition or by the occasional fires that destroyed thousands of valuable federal records. Rather, it was the smell of pork that convinced congressmen to lay partisan and philosophical differences aside and pass the necessary authorization and appropriation during the first half of 1926. Ultimately, legislative victory came because of Coolidge's concern that governmental agencies and their historical records be properly housed in the nation's capital. Congressmen agreed only after expanding the building program to include millions of dollars for post offices in their home districts.

The hero of the twenty-year struggle is clearly Jameson, historian, promoter, lobbyist, technician, and coordinator. He showed the persistence of Sisyphus and the patience of Job in overcoming the frustrations of many defeats, each occurring just as victory seemed assured. Legislators authorized but

did not appropriate; Congress approved initial planning, but underfunded it. When his suggestion that the architects inspect European archives before developing their plans was incorporated into the legislation, the advent of World War I required him to support new legislation to remove that stipulation. While Jameson had at least the lukewarm support of Presidents Roosevelt and Taft, he turned with more hope to his old Johns Hopkins graduate school colleague and fellow scholar, Woodrow Wilson, only to be rebuffed by a president distracted by foreign affairs. Such episodes delayed the painfully slow birth of the National Archives. Had Jameson realized just how long his dream would remain unfulfilled, he might have despaired. As it was, he died just three years after the future of the National Archives building was assured. His experience should be better understood by contemporary archivists and historians who have periodically, but only half-heartedly, agitated for independence of the National Archives and Records Service from the scandal-plagued General Services Administration.

This thoroughly researched study is the product of an architect, archivist, and military historian writing at the end of a career that included a quarter century at the National Archives. Victor Gondos, Jr.'s background as an army officer and his lifelong interest in the military served him well in his posts at the National Archives as chief of the old army and Civil War branches, and eventually as a chief of the army and navy branch. Although he began coursework for a doctorate in the late 1950s, he did not complete his dissertation until 1971 at age sixty-nine, after he had retired from federal service. Gondos did not live to see his study published, but former archivist of the United States James B. Rhoads was faithful to Gondos's original work in this carefully edited version. Together with Donald McCoy's award-winning history of the National Archives and H. G. Jones's *The Records of a Nation*, Gondos's book provides us with a very complete record of the origins and development of an important twentieth-century cultural and historical institution. We still lack, however, a scholarly history of the archival profession in the United States.

NICHOLAS C. BURCKEL
University of Wisconsin,
Parkside

TOM D. CROUCH. *A Dream of Wings: Americans and the Airplane, 1875–1905*. New York: W. W. Norton. 1981. Pp. 349. \$15.95.

This book adds significantly to the limited scholarly literature on the history of flight. Its author, Tom D. Crouch, a historian and curator at the National Air and Space Museum, deepens our understanding of the origins of the airplane in a clear, nicely paced

narrative. Addressing in depth the human, scientific, and engineering sides of the topic, Crouch achieves an effect that compares favorably to that conveyed by the National Air and Space Museum to its visitors.

Crouch follows the trail broken by the distinguished British historian of flight, Charles H. Gibbs-Smith, who put the origins of the airplane into their proper international context and firmly established the Wright brothers as the first inventors to develop the essential capabilities of a successful airplane. The distinctive contributions of Crouch in building on the work of Gibbs-Smith stem from his excellent use of both the personal papers of American experimenters and also the many files of aeronautical and other journals and newspapers chronicling their story.

Four Americans dominate the narrative: Octave Chanute, the catalyst, confidant, and sometimes patron to American experimenters as well as the link to their European counterparts; Samuel P. Langley, the demanding secretary of the Smithsonian and, to nearly all of his interested contemporaries, the most likely individual to succeed in building a successful airplane; and, the two men who did succeed, the formally untrained but marvelously insightful, practical, and persistent Orville and Wilbur Wright. Although Americans dominated experimentation during many of the years described in this book, Europeans such as Otto Lilienthal contributed to their success, and Crouch does not forget this. Importantly, also, he succeeds in integrating into his study appraisals of the work and personalities of the American inventors Augustus Herring, Gustave Whitehead, and John Montgomery, whose failures, like that of Langley, make the success of the Wrights all the more remarkable. In the perspective developed by Crouch, the readiness of Herring to compete for the first U.S. Army contract to build an airplane has greater plausibility than earlier accounts have suggested.

A more substantial bibliography and a commentary on sources would have made this book even more useful. Crouch mistakes H. A. Hazen for W. B. Hazen at one point and at other points incorrectly identifies the positions in the army of Nelson Miles and William Glassford. Still, these minor criticisms should not detract from the many good qualities of this book. To point to just one more admirable feature: no previous author has succeeded so well in placing side by side the stories of Langley and the Wrights as they moved toward the climax that would open the early years of the twentieth century with an achievement whose significance historians are only now beginning to study systematically.

ALFRED F. HURLEY
North Texas State University

ELEN C. SINGH. *The Spitsbergen (Svalbard) Question: United States Foreign Policy, 1907–1935*. Oslo: Universitetsforlaget; distributed by Columbia University Press, New York. 1980. Pp. iv, 244. \$23.00.

The transition of Spitsbergen from a "terra nullius" into a Norwegian possession (renamed Svalbard in 1925), and, despite the Monroe Doctrine, the attempts of American businessmen from 1907 onward to involve Congress, the State Department, and United States foreign policy—these are the subjects of this scholarly research by Elen C. Singh.

Singh's 146 pages of detailed analysis examine the growth of a single New England commercial enterprise, the Arctic Coal Company, in the archipelago and its effective lobbying in Congress and the influence it wielded on State Department policies from 1907 until the sale of its mining rights to a Norwegian syndicate in 1916. Yet the State Department continued to be interested in the status of the islands as Russia (and, later, the Soviet Union), Norway, and others asserted mining, fishing, and hunting claims there. American concern did not subside until the ratification of the Spitsbergen Treaty and eventual Soviet adherence in 1935 after an eleven-year delay due to American nonrecognition of the Soviet government.

In an epilogue, the United States is shown as a continuing and interested bystander in the affairs of Svalbard. The international importance of this arctic archipelago has been heightened by the development of the continental shelf doctrine and the fisheries protection zone, as well as the growing possibility of finding oil and gas, and the implications this would have for the maneuverability of the Soviet fleet in the area.

First called Svalbard in 1194 in the Icelandic annals, and, according to the arctic explorer, Fridtjof Nansen, probably visited by Norsemen from the ninth to the fourteenth centuries, the archipelago was rediscovered on June 17, 1596, and named Spitsbergen by the Dutchman Willem Barrens while he was attempting a Northeast passage. Russians also visited the islands to hunt and fish, but because of the ice pack they thought it was part of Greenland and called it Grumant, a corruption of Greenland.

Americans became interested in 1903 when John M. Longyear and Frederick Ayer of Boston took samples of coal and decided that mining was feasible on the largest island, still called Spitsbergen. They organized the Arctic Coal Company, a West Virginia corporation, staked out enormous claims, and then sought United States' protection for their properties in a "terra nullius" where no law reigned. Ingeniously they drew a parallel between their mining in this arctic no man's land and the authority given in the Guano Islands Act of 1856 virtually to

annex unclaimed islands. Singh follows the lobbying of Longyear to amend the act, the failure of the House of Representatives to approve the Senate bill, the alarm in Norway, and the resort to three conferences—1910, 1912, and 1914—in the last of which the Americans limped badly for lack of any staff "thoroughly conversant in French," the language of negotiations.

World War I foreclosed agreement. But at Versailles in 1919 the victorious Allies, while assigning the Aaland Islands and Schleswig questions to the League of Nations, settled the Spitsbergen question by allowing Norway to annex the archipelago as restitution for shipping losses. The official signing of the Spitsbergen Treaty took place on February 9, 1920, with representatives from the United States, France, Great Britain, Italy, and Japan from the belligerent nations and Denmark, Norway, Sweden, and the Netherlands from the neutrals. Not until 1935 did the United States permit the Soviet Union to sign. Svalbard is unique in that the Soviet Union has equal rights with other signatories to develop and exploit the resources and to coexist in a demilitarized setting.

Besides being a case study of private commercial involvement in official policy making of the United States, this book is a scholarly bit of recent Scandinavian history of which, because of ecology and strategy, we should be more knowledgeable.

G. BERNHARD FEDDE
Portland State University

PHILIP H. BURCH, JR. *Elites in American History*. Volume 2, *The Civil War to the New Deal*. New York: Holmes and Meier. 1981. Pp. xii, 496. Cloth \$38.50, paper \$19.75.

It is rather difficult to discern just what this book is all about. Part of a three-volume effort (volume 3 was published first, volumes 1 and 2 appeared simultaneously), Philip H. Burch's essential argument seems to be that major economic power in America has had a disproportionate influence in the running of American government throughout most of its history. His method of proof is to list the cabinet, ambassadorial, and Supreme Court appointments of each president from Lincoln to Hoover. The backgrounds of these men are then investigated to determine if they were part of a "big business elite." Burch's rationale for all this is that "a President may be judged by the type of men he picks to serve as his chief aides and advisors" (p. 311).

Fair enough. But to what end? Burch never makes clear (at least in volume 2) just what the impact of these appointments has been on public policy, on economic changes, or for the nation as a whole. For example, a couple of administrations

(Andrew Johnson and Woodrow Wilson) had disproportionately lower numbers of men from elite backgrounds, but Burch goes to great lengths to demonstrate that this was largely meaningless, that in fact both administrations were quite cozy with big business interests. Well, if it does not make any difference what percentage of the appointees are from elite backgrounds, why does he bother making the lists in the first place?

After 310 pages of these mind-numbing lists of presidential appointments (followed by a 156-page appendix that gives virtually identical information in tabular form) Burch's 15-page summary seems to trivialize the data even further. From this mass of data he makes four conclusions, three of which are largely truistic and obvious. He shows that a larger percentage of the office holders came from New York City than was true in the early nineteenth century. Second, the South, especially Virginia, suffered a sharp decline during this period. This is not surprising since these were mostly Republican administrations. Third, more of the appointees were college, especially Ivy League, graduates, and Supreme Court justices were better educated than other appointees.

Burch's final, and most important conclusion, is that appointees after the Civil War had less governmental experience than before and that they were drawn more heavily from the worlds of business and corporate law. Yet he admits that there were significant variations in the percentages of business leaders appointed from administration to administration but that these differences had little discernible effect on the policies of those administrations.

Burch's efforts seem directed at destroying a straw man, that American historians are utterly unaware of business influence in governmental decision making. But this is no longer the case, if it ever was. What we need to know is the degree to which "big business" was united on a program of political economy over this 63-year period, how much of this program they were able to put into operation, and how they were able to do it. A "positional analysis," simply listing directorships and other affiliations does little to advance this end.

JOHN N. INGHAM
University of Toronto

SUSAN WARE. *Beyond Suffrage: Women in the New Deal*. Cambridge: Harvard University Press. 1981. Pp. 204. \$18.50.

That women played a central role in certain New Deal agencies has been known for some time. What Susan Ware has provided in *Beyond Suffrage* is the first detailed analysis of the nature of that involvement. Focusing on the roles and relationships of

twenty-eight women whom she identifies as key to social progress for women during the New Deal. Ware argues not only that women profoundly shaped New Deal social programs but also that in the 1930s many of women's goals beyond suffrage were realized.

The New Deal women were led by Mollie Dewson, head of the women's division of the Democratic party, Ellen Sullivan Woodward, head of the women's and professional projects for the Works Progress Administration, Frances Perkins, secretary of labor, and of course Eleanor Roosevelt. According to Ware, they formed a generational cohort as well as an activist "network." Born around 1880, they constituted a successor generation to Jane Addams's generation of women reformers. Like their predecessors they participated in the social justice wing of the Progressive coalition and became leaders in the settlement house movement. They were important in the revitalization and victory of the suffrage campaign, and in the 1920s they led the crusade for federal legislation in areas like the child labor amendment. Many were involved in the Consumers' League and the Women's Trade Union League and had been close to the Roosevelts in New York politics. Dewson and Eleanor Roosevelt were largely responsible for bringing them into administrative positions. From them, according to Ware, they played a key role in establishing provisions for women under, for example, the National Industrial Recovery Act of 1934 and the Social Security Act of 1937. By the late 1930s, however, their own retirement and the growing conservatism of the federal government brought a steady decline in their authority and in the general numbers of women in government. In the final analysis, they marked the end of Progressive women in politics and gave way to a generation of women who had neither their sense of mission nor their political skills.

Focusing on such a small group of women has necessarily limited the scope of Ware's book. She reveals little about the general situation of women during the New Deal, about the success of New Deal programs for women, or about the interaction of federal women administrators with women's organizations in general. Her enthusiasm for her subjects has led her to accept uncritically their own positive interpretation of their work. One questions her conclusion that substantial goals for women were achieved during the 1930s.

Still, although the book is a limited exercise in administrative history, Ware offers an interesting perspective on the notion of generational cohesion as a force in social change—an idea frequently cited by scholars but rarely utilized in any systematic manner. And at a time when women are grappling with the questions of preferment and association as a means of advance, Ware has provided an interest-

ing perspective on how a women's "network" worked in the past.

LOIS W. BANNER
George Washington University

MARION CLAWSON. *New Deal Planning: The National Resources Planning Board*. Baltimore: Johns Hopkins University Press, for Resources for the Future, Washington, D.C. 1981. Pp. xxii, 356. \$32.50.

Marion Clawson's *New Deal Planning*, in which he undertakes to "analyze and describe the National Resources Planning Board and its direct predecessor agencies," is not the kind of book that most historians would write. Its format and tone are essentially those of the government report rather than the historical monograph. Its use of historical scholarship is meager, and its concerns are less with historical problems than with evaluating agency output and contributing to current policy debates. But if this is not a "history book," as one usually thinks of that genre, it is a book that historians will find useful and informative. In it Clawson draws upon agency materials, extensive interviewing, and his experience in government service to provide both a convenient summary of NRPB activities, 1933–43, and much fresh information about the agency's origins, internal dynamics, and political demise.

In presenting his material, Clawson has chosen to group it in five major sections, focusing in turn on the agency's origins, its evolution and operation, the nature and value of its work, the place it came to occupy in the governmental structure, and its demise and legacy. Such a structure enables him to examine his subject from several interrelated angles, thus building the support needed for statements made in his introduction and concluding analysis. But it also makes for much overlap and repetition. Not infrequently, the reader finds himself being told essentially the same thing for the third or fourth time.

For this reviewer, the most interesting and valuable sections of the book were those on pre-New Deal precedents, the promotion of subnational planning, and the failure of the NRPB to survive. In these Clawson sheds new light on the often neglected linkages between New Era and New Deal planning, on the growth in the 1930s of regional, state, and local planning, and on the inability of the NRPB to balance apolitical expertise with the political connections needed for budgetary survival. Also fascinating is his discussion of the committee structures through which the agency conducted its early and most successful investigations, although here the parallels with similar structures in the 1920s are not drawn.

In his final assessment, Clawson judges the NRPB to have been generally successful in its role as an "idea stimulator" but peripheral to the development of the New Deal as a whole and only occasionally successful in its attempts to function as an interagency coordinator and policy planner. It was a mistake, he concludes, to have assigned it multiple roles, some of which were incompatible with its administrative structure and support system. And while refusing to take a position for or against a reconstituted NRPB, he strongly suggests that future planners should learn from this and make certain that the roles of their organizations are both clearly defined and fully compatible with administrative resources, legislative bases, and operational methods.

Clawson's study is one that can be read with profit both by those interested in the history of the New Deal era and those seeking solutions to current political and economic problems.

ELLIS W. HAWLEY
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JOHN R. PETROCİK. *Party Coalitions: Realignment and the Decline of the New Deal Party System*. Chicago: University of Chicago Press. 1981. Pp. 215. \$20.00.

Historians and political scientists have been preoccupied with political party "realignment" for over a quarter of a century. Many definitions of realignment have been offered: as enduring shifts in the relative electoral strength of the parties, as changes in the partisanship of segments of the electorate, as structural recombinations of the social groups comprising the party coalitions, and so forth. For the most part, the effort has been aimed at giving quantitative structure to the trends and changes in the behavior of a mass electorate over time. John R. Petrocik endeavors, impressively, to broaden, refine, and improve the way this electoral phenomenon is measured. His discovery is that turnout is a major factor in the realignment process.

According to Petrocik's theory and data, some realignments of the past have resulted from the mobilization and increased turnout of previously inactive citizens, that is, the participation of urban Catholics who were drawn into the voting process by Al Smith's candidacy in 1928. Other realignments, particularly the present one, appear to be caused "largely by a decline in support for both parties" (p. 158). Fluctuation in the size of the electorate occurs differentially in particular social groups, affecting the partisan bias of the group and the electoral consequences of such group shifts.

Petrocik also demonstrates that the profile of social groups comprising party coalitions changes with realignments. These changes are related to

changes in the agenda of public issues that, in turn, affect group perceptions of the parties' proximity to group attitudes and policy preferences. Added up, these shifts in group partisanship and perception may or may not alter the overall competitive balance between parties, yet they constitute a realignment nonetheless.

The author has developed measures of the partisanship of fifteen demographically definable social groups that appear most relevant to party coalitions—Northern white Protestants of various social statuses, native Southerners of various statuses, immigrants to the Deep South, Polish and Irish Catholics, blacks, Jews, union members, and so forth. Using survey data derived from sixteen thousand interviews over a twenty-year period, Petrocik found significant shifts in the turnout, party bias, and issue perceptions of ten of these groups. He is then able to report in what ways the Democratic coalition under Jimmy Carter was different from that of the New Deal. As a consequence of decline in support of the Democratic party, the greatest changes are seen among affluent Southern whites in both the Deep South and the border states as well as among lower-status whites in the Deep South—a shift to the Republican side. Outside the South, however, there has been a general decline in voter support of the Republicans, particularly among high- and middle-status Protestants. Blacks and Jews have increased their pro-Democratic bias, although Jews have tended to become more independent. Other groups are similarly tracked in this exercise in quantitative history, and it becomes clear that Carter's coalition is indeed different from the coalition of the New Deal.

Thus, Petrocik's theory of party and electoral realignment is substantially supported by his data. His attention to fluctuations in turnout and independence is innovative and significant. His approach and findings should have important practical implications for leaders of the party battle as they try to identify components of their constituencies, deal with the volatile issue agenda of American politics, and harness group perceptions into a winning party coalition.

RALPH M. GOLDMAN
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CHARLES W. JOHNSON and CHARLES O. JACKSON. *City Behind a Fence: Oak Ridge, Tennessee, 1942–1946*. Knoxville: University of Tennessee Press. 1981. Pp. xxiii, 248. Cloth \$18.50, paper \$9.50.

Charles W. Johnson and Charles O. Jackson, two social historians at the University of Tennessee, have attempted the by-no-means easy task of writing a history of Oak Ridge, one of three towns built by

the U.S. Army Corps of Engineers during World War II to accommodate the families of those who built the first atomic bomb. At the outset the authors seem aware of the possibility of writing a social history of Oak Ridge. The book in fact contains much information of interest to social historians. Johnson and Jackson see the fence surrounding the community not just as a security barrier but also as an influence on the social characteristics of the town. As social historians the authors have a keen eye for the various manifestations of racial and sexual discrimination that were prevalent in east Tennessee during the early 1940s. They have also been successful in defining the cultural differences between Oak Ridge residents and the "hillbillies" of the surrounding countryside. Different expectations and standards in terms of education, personal conduct, and public morality all created tensions across the security barrier. The book, however, is disappointing as social history mainly because it lacks depth in analysis. The authors had at their disposal exceedingly rich documentation on all aspects of the town's development, yet they chose not to undertake the kind of detailed statistical analysis that would have supported and refined their conclusions.

Johnson and Jackson also recognized the possibility of writing in the context of urban history. In the introduction they explore the utility of several analogies from earlier examples of community development, but they reject all these comparisons as inadequate: ". . . nor did there exist any meaningful historical precedent for the Oak Ridge experience" (p. xxiii). Be that as it may, Oak Ridge did not evolve from thin air. The authors tell us quite correctly that Oak Ridge was not designed to be a model city or a social experiment but rather "to approximate a typical American small town as much as possible" (p. 35). But what did these words mean? The authors describe the criteria used by professional planners and the army in designing the town. It is clear that the plan was based on assumptions about the kind of town that "average" Americans would find acceptable, but the authors give not a clue about the origins of these assumptions. Is it possible that a careful exploration of this subject would tell historians something significant about concepts of urban development prevailing during the middle decades of this century?

It seems that the authors chose to pass over such questions and to settle for a popular account in the genre of local history. In this they have succeeded. The book can be read with pleasure and profit by the residents of Oak Ridge. For historians with broader interests, however, the book merely suggests the opportunity for further studies of greater import. We can hope that Johnson and Jackson will return to the voluminous and rich data that they

have collected to produce monographs that will answer some of the important questions that this book brings to mind.

RICHARD G. HEWLETT
History Associates, Inc.

JAMES H. JONES. *Bad Blood: The Tuskegee Syphilis Experiment*. New York: Free Press. 1981. Pp. xii, 272. \$14.95.

Southern blacks blamed "bad blood" for many of their illnesses. The Tuskegee study, which concentrated on one particular kind of bad blood—syphilis, was an experiment conducted by the United States Public Health Service. Its aim was to observe the natural course of the disease and to discover whether or not that course was different in blacks than in whites. Its method was to deprive 399 syphilitic blacks of treatment for their condition over a forty-year period (1932–72).

Because of the nontherapeutic nature of the experiment, men died, went insane, and became blind. Yet the subjects were told only that they had "bad blood" and that the "government doctors" who examined them periodically were treating their condition. This book is devoted to examining how such an experiment could have come about, how it was carried out, and how and why it ended.

In order to accomplish this, James H. Jones sketches the attitudes of nineteenth- and early twentieth-century physicians toward race and disease, discusses the problem syphilis became in the South (Macon County, the site of the experiment, had a prevalence rate of 36 percent), and then shows how the study originally planned for six months to a year became instead the longest running nontherapeutic experiment in medical history.

Naturally, the nature of bureaucracies in general, the PHS in particular, and the arrogance and isolation from lay control of the American medical profession all come under scrutiny. But the work is essentially concerned with medical ethics, and consequently much of Jones's preoccupation is with the lack of sensitivity to moral and ethical questions, which those with knowledge of, let alone those involved in, the study revealed.

The Tuskegee study was scarcely a secret. Generations of PHS physicians worked on it, various findings were reported in leading medical journals, and all Macon County physicians were made aware of it so that they would not "contaminate" a subject by treating him. Yet no moral objections to the study were voiced until the very last years of its existence, and in the end the study was terminated by the hot glare of publicity rather than by moral qualms on the part of the PHS.

Jones himself played a role in the events flowing in the wake of the study's demise as a researcher for

the attorney who brought a class action suit against the government on behalf of still living subjects and the families of those who had died. This experience in turn facilitated access to many of those connected with the experiment whom Jones interviewed. These interviews, interwoven with official records (which Jones was able to consult only after he threatened his own suit) constitute the bulk of the book's impressive documentation.

As a study in medical ethics this is a provocative and disturbing book. As social history the work is a well-written examination of the plight of poor blacks in the South and the repercussions of that plight in terms of health. As a medical history, the book exposes a particularly dismal chapter in America's recent past. If Jones can be faulted for anything, it would be that he fails to satisfy the reader's curiosity as to the question of what scientific results, if any, were yielded by the experiment and whether it was determined that syphilis did in fact run a different course in blacks, as have other diseases with which they have had an abbreviated (relative to whites) historical contact. With this single exception the book deserves to be termed definitive.

KENNETH F. KIPLE
Bowling Green State University

C. A. MACDONALD. *The United States, Britain, and Appeasement, 1936–1939*. New York: St. Martin's Press. 1981. Pp. xi, 220. \$25.00.

TERRY H. ANDERSON. *The United States, Great Britain, and the Cold War, 1944–1947*. Columbia: University of Missouri Press. 1981. Pp. xi, 256.

These two books deal with important developments in Anglo-American relations before and after World War II. Both are based on thorough research in the Foreign Office and State Department records of the two countries. C. A. MacDonald stays close to the documents to write an objective but narrowly focused diplomatic history; Terry H. Anderson consults a far wider range of sources to give his account greater breadth at the expense of exploring some significant topics in detail.

Two themes run through MacDonald's careful study of British-American relations from 1936 to 1939. The first is the unsuccessful American attempt to influence the course of British policy toward Nazi Germany. At first, Franklin D. Roosevelt hoped to persuade Prime Minister Neville Chamberlain to pursue a joint policy of economic cooperation, and then, after Munich, the opposite course of checking German aggression. Chamberlain, aware that FDR was not willing to go beyond economic encouragement and moral support, refused all of Roosevelt's initiatives. As a result, the United States was unable to alter British policy in the months prior to the outbreak of war in 1939.

Equally important, MacDonald's account reveals the mutual distrust that characterized relations between the two nations who have prided themselves on a "special relationship" in the twentieth century. The British were disdainful of American attempts at economic cooperation without political commitment. It was "always best and safest to count on *nothing* from the Americans except words," Chamberlain wrote privately in 1937 (p. 48). Even in the summer of 1939, when FDR was dangling offers of arms and money in return for a stiffer British policy, Chamberlain feared that an isolationist America could not be counted on for political and military support. And Roosevelt was equally suspicious of the British prime minister, seeing him as representing City of London financial interests bent on peace at any price. Thus Sumner Welles reported FDR as commenting in August 1939 on the futility of the United States "trying to lend its moral support to a power which is deliberately intent on suicide" (pp. 164–65). This reciprocal distrust poisoned relations between the two English-speaking democracies and thus helped speed the war both hoped to avert.

In his more ambitious study of the early Cold War, Terry H. Anderson tries both to fill in an important gap in the existing literature and to show specifically the impact of British diplomacy on the development of the American policy of containment. He is much more successful in the first endeavor than in the second. Using the recently opened British materials in the Public Record Office, Anderson describes the English attempts to alert the United States to the Soviet menace from 1944 to 1947. According to Anderson, Roosevelt, who preferred the role of mediating between Churchill and Stalin, finally was persuaded to adopt a firm policy toward Russia in the early months of 1945. But then his death led to a naive Truman reverting to FDR's cooperative policy until the combination of British warnings and Russian intransigence finally persuaded him to opt for containment in late 1946 and early 1947.

Crucial to Anderson's account is an evaluation of the impact of British diplomacy on American foreign policy, and this is precisely where the author's sources are least helpful. He relies primarily on the British records, which are filled with evidence of Foreign Office warnings of Russian duplicity, but offer little insight into how American diplomats reacted. He is quite honest in stating that at critical moments, especially in 1946, brutal Russian action in Eastern Europe had far greater impact on the United States than did British diplomacy. Indeed, he finally concludes by absolving the British of any responsibility for beginning the Cold War, claiming that it was Russian behavior that ultimately forced the United States to take a firm stand in Europe.

In his preface, Anderson aligns himself with

neither the orthodox nor revisionist schools, but rather chooses the label of postrevisionist. His account, however, suggests that he stands squarely in the orthodox camp. Roosevelt is portrayed as an overly trusting leader who placed all his hopes for peace on postwar Russian cooperation until Churchill finally aroused him to the Soviet danger in early 1945. In contrast to the revisionist view, Truman's greatest mistake is not reversing FDR's soft line toward Russia but rather continuing it from mid-1945 through early 1946. The British are seen throughout as wise and reliable, grasping the Soviet danger even in the midst of World War II but being forced to wait until events finally bring the Americans to their senses. Relying heavily on Joseph Jones for his classic Cold War account of the Truman Doctrine, Anderson concludes that the British played the role of catalyst in the final adoption of the containment policy.

Each book makes a useful contribution to American diplomatic history. Anderson's study is lively and entertaining, with the virtue of subordinating facts and details to the larger outlines of policy. MacDonald's book lacks drama and clarity, but his careful recounting of diplomatic exchanges faithfully recreates the confusion and uncertainty and above all the mutual misunderstanding that characterized decision making in both England and America on the eve of World War II.

ROBERT A. DIVINE
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RUSSELL F. WEIGLEY. *Eisenhower's Lieutenants: The Campaign of France and Germany, 1944–1945*. Bloomington: Indiana University Press. 1981. Pp. xviii, 800. \$22.50.

Russell F. Weigley's earlier writings prepared the way for this outstanding book. His histories of American military thought, of the United States Army, and the American way of war all furnished the foundation for this highly professional study of command and operations in northwest Europe, 1944–45. In an authoritative weaving together of high-level command decisions with the details of ground and air operations at lower levels, he has written the best account that we have of the World War II campaigns from Normandy to the Elbe.

He has made effective use of the research that has gone before. (And he has given full credit in his footnotes.) Basing his narrative squarely on the official Allied histories, he has combed the sources in print and has added material from some diaries and journals that have not been used before. Unlike some recent authors who have been interested mainly in trivia and snickering asides, he has used this material for a fresh look at old controversies.

He adds color to his narrative by careful reminders of the earlier history of famous divisions and by noting that the World War II armies followed historic trails in their campaigns.

Weigley has lacked the wealth of biographical and autobiographical material to permit him to rival Douglas Freeman's *Lee's Lieutenants* in writing of all of Eisenhower's subordinates. But there is enough to show Eisenhower's relationships with his chief commanders and to judge something of their abilities. He finds Eisenhower and Bradley often cautious and unimaginative, is balanced in judgment on Patton, and is harsh on Hodges's leadership in the field. Drew Middleton has called him overly critical of Montgomery. But Weigley carefully states that Britain's waning manpower made for caution. His strictures on Montgomery's behavior toward his colleagues are well documented.

Weigley assigns blame equally among top commanders for failure to plan adequately for exploiting victories after the opening phases in Normandy and criticizes the American gamble on manpower. He has a judicious analysis of the reasons for failure at Arnhem and gives an even-handed judgment on the broad versus narrow front debate. There is ground for disagreement on his conclusion that Bradley erred by not bypassing the Ruhr and sweeping toward Berlin. Here he may have been overpersuaded by General James Gavin's recent book.

Of particular importance is the introductory material on weapons, doctrines, training, and experience of the contending forces. His assessment of weapons corrects a frequent omission in earlier books on the campaigns. His is the first in-depth operational account of the fighting in northern Europe to incorporate ULTRA information in its running story.

Weigley has handled with considerable success the tricky problem of going repeatedly from high-level planning and decision making to actions on a limited front. And no one has ever been able to avoid a glazed-eye look on the part of many general readers. However, even the initiated will get caught up in the excitement generated by Weigley's stories of the pursuit across France and Belgium, the drive to the Rhine, and the final sweep across Germany as enemy resistance crumbled.

FORREST C. POGUE
Smithsonian Institution

ROBERT H. FERRELL, editor. *The Autobiography of Harry S. Truman*. Boulder: Colorado Associated University Press. 1980. Pp. xiii, 153. Cloth \$10.00, paper \$4.95.

ROBERT H. FERRELL, editor. *The Eisenhower Diaries*. New York: W. W. Norton. 1981. Pp. xvii, 445. \$19.95.

ROBERT H. FERRELL, editor. *Off the Record: The Private Papers of Harry S. Truman*. New York: Harper and Row. 1980. Pp. xiv, 448. \$15.00.

Two of these books, *Off the Record* and *The Eisenhower Diaries*, can be dealt with seriously. The third one, *The Autobiography of Harry S. Truman*, cannot. It adds little, if anything, to the material available elsewhere and is mistitled in a way that could lead an unsuspecting bibliographer astray. "Autobiographical fragments" might be more accurate, although that is stretching it. Even with the first two, taking them seriously—and together—poses some problems and many fascinating insights. Let us take the latter first.

While Truman and Eisenhower were contemporaries and involved with one another at important intervals for many years, one might not be tempted to think of them that way. Yet these books open the way to comparisons that are at least interesting, and at best possibly important. Neither man directed his career toward the presidency. Both men rejected the notion of themselves as men with such ambitions. Both considered themselves unworthy, and both took the position either as a duty or an act of God, disliking large parts of it, particularly the partisan aims of others, resenting the frequent absence of support from those they believed ought to have known better, and maintaining attitudes toward the press that ranged from disdain to loathing. Even after their respective stints in the Oval Office, both continued to suspect one another's motives and interests, as well as those of their ultimate successor, John Kennedy. Both men felt themselves poor public speech makers and mistrusted eloquence in others. Not until he discovered the effectiveness of his "off the cuff" speaking did Truman gain confidence, while Eisenhower's private comments have a precision and thoughtful clarity one would not have been led to expect from his public utterances. Truman referred to Franklin Roosevelt as a "fakir," while both men appeared to find Stevenson glib and untrustworthy. They distinguished between "ideas" and "words" in a fashion that would delight a medieval Platonist.

The texts of both books consist primarily of diary entries. Eisenhower's go back to his stint in the Philippines in the mid-1930s under Douglas MacArthur, and "under" is the proper preposition. Entries are quite sporadic, but many are full and extremely revealing. His was a far more systematic and analytic mind than critical accounts of his career suggest. Eisenhower detailed the administrative behavior of others with a shrewdness and judgment that make his position as leader far clearer than it has tended to be. One gets a strong sense of his view of the relation between public purposes and objectives, on the one hand, and personality and personal ambition on the other. He used family and upbringing as

a moral touchstone against which to measure his own actions and intentions and through which to judge the behavior of others. And he was constantly sitting in judgment, cataloguing the administrative personalities of everyone with whom he dealt. There was remarkably little sense of history (in contrast with Truman for whom the past was virtually a collection of parables), yet a surprisingly complex set of ethical conceptions and abstractions were used to examine what others said to him, to list his own responses, and to raise further questions for him to answer. He was respectful of Churchill and Roosevelt but well aware of their weaknesses, particularly their inability to make what he considered hard and systematic decisions about others. Once in the presidency Eisenhower followed the same analytic methods, and the diaries are alive with his interest in the details of the projects he explored and his judgments about others who appeared before him. Instead of being politically inept, as some have pictured him, he was much closer to the classic Progressive in his attitudes toward politics. The use of patronage to mask or excuse incompetence was a recurrent target of his anger.

Off the Record adds a great deal to our understanding of the personality of the man who succeeded the complex FDR. It consists of personal letters to family and close friends, an impressive number of diary entries on a wide range of subjects, and a wonderful sprinkling of unsent letters to newsmen, opponents, and a delightful one to his wife after a Christmas Day squabble. Truman justified an extraordinary number of his actions by a laying out of historical models ranging back to antiquity, accurate only in terms of the kind of Victorian historiography with which he was acquainted (he took pot shots at Charles Beard, whom he thoroughly disliked). He was a political professional in his accounting of the reasons for his actions and opinions, reciting past errors and accomplishments of his own and others as guides and proofs. Truman was a quintessential American Christian, but an evangelist, not a theologian. His simplicity was his greatest support, his capacity to see black and white, without gray, let alone color, and it is at times disturbing.

What must also be said, however, is that the books are not designed and edited with a scholarly audience in mind. That is not necessarily a harshly critical comment since Robert H. Ferrell's headnotes and running commentary are lively, partisan, and generally helpful to those who do not know the period or need to be reminded of their past commitments to either man. There are few real errors (Eisenhower, for example, was not prevented from running for a third term by the Presidential Succession Act of the Truman administration but by the Twenty-second Amendment, a far more mischievous and constitutional reality), and the footnotes,

while generally useful, are sometimes so brief in their identification of individuals as to be absurd. To call Russell Leffingwell simply "A Lawyer" when he visited Eisenhower to urge presidential candidacy on him is woefully misleading. A reader who does not recognize one of the major powers of national finance will be told nothing, and one who does will stutter in amazement. It would be like calling Eisenhower "A Soldier."

More important, one cannot be certain about omissions. Truman scholars will be aware of the fact that the documents selected present him in a somewhat better light on some major issues than others that could have been used. The gaps in the Eisenhower materials could at least be explained, if the documents do not exist. The resignation of Sherman Adams is not touched upon at all. Despite the fullness of his comments on his relations with Congress and with his cabinet members, there is relatively little on the extensive McCarthy battle. These books will not substitute for research monographs or for proper scholarly editions of papers. They are, nonetheless, good reading for those who want to think their way through the period again, and read together they are an exciting experience, indeed.

BARRY D. KARL
University of Chicago

BLANCHE WIESEN COOK. *The Declassified Eisenhower: A Divided Legacy*. Garden City, N.Y.: Doubleday. 1981. Pp. xxiv, 432. \$17.95.

Blanche Wiesen Cook is the kind of historian who believes that truth remains embedded in mountains of classified materials until the unflagging efforts of determined researchers finally disclose what really happened. Thus she is convinced that others have "consistently belittled and ignored" the "fundamental realities of the Eisenhower years" because "our historical documents" have been "classified and hidden." The view of Dwight D. Eisenhower as "a nowhere man, doing nothing on the presidential road to retirement," has been allowed to persist (p. vii). That will all be news to such students of Eisenhower's presidency as Herbert Parmet, Douglas Kinnard, Robert Divine, and Richard Aliano. Certainly it is news to this reviewer. Cook, though, seems unaware of or unconcerned with most of the professional scholarship on Eisenhower produced over the past ten years.

The Declassified Eisenhower is a stridently phrased, left-revisionist overview of the period from roughly 1945 to 1960. It is an awkwardly balanced book. In an effort fully to account for Eisenhower's "political education," Cook gives close to half of her text to the years before he became president. Another seventy-five or so pages go to one episode in Eisenhower's

presidency, the Guatemalan coup of 1954. The remainder of the book consists of a loosely written account of Eisenhower's leadership in promoting the global expansion of American political, cultural, and, most of all, economic power. A shrewd planner and executor, the willing agent of "the American Century" (a phrase he himself evidently never used), Eisenhower "worked largely undercover. America's most popular hero was America's most covert President" (p. vi). Spearheaded by the Central Intelligence Agency and by C. D. Jackson, Eisenhower's psychological warfare chief, the administration militantly and multifariously waged the Cold War, at the same time that Eisenhower resisted escalation of the nuclear arms race and genuinely sought a basis for peace with the Soviets.

That is scarcely an original interpretation. And despite Cook's assiduous research in the documents, many of which she secured access to under the Freedom of Information Act, she turns up nothing startling about the way Eisenhower and his subordinates used power. At the same time, her thick footnotes often do not support her assertions. For example, J. Edgar Hoover persuaded Eisenhower that civil rights protests in the Southern states were "the work of communist terrorists" (p. 174). Francis Gary Powers's ill-fated U-2 mission was the Eisenhower administration's way of sabotaging the 1960 Paris summit (p. 215). After the Guatemalan coup, U.S. Ambassador John Peurifoy brought Colonel Castillo Armas a long list of Communists to be shot (p. 286). Eisenhower "presided over the formation of every major institutional change that enthroned the military-industrialists" (p. 300).

Progressive, humanitarian sentiments do not compensate for a poorly constructed book, one marked by runaway rhetoric and shaky conclusions (and a plethora of dangling modifiers). In her preoccupation with the secrets that the documents supposedly hold, Cook has confused more than clarified the history of the Eisenhower years.

CHARLES C. ALEXANDER
Ohio University

MICHAEL BALFOUR. *The Adversaries: America, Russia, and the Open World, 1941-62*. Boston: Routledge and Kegan Paul. 1981. Pp. xv, 259. \$25.00.

The field of Cold War studies badly needs an interpretive synthesis written not from the predictable perspectives of Americans or Russians but from those of third parties whose histories the great-power confrontations of the post-1945 era did so much to shape. The distinguished British historian Michael Balfour has attempted such a synthesis in

this volume. Unfortunately, the results are disappointing.

Balfour intends his book to provide "an interpretive framework for the period from the early 1940s to the early 1960s, in the belief that a pattern is beginning to be visible in that period which it is still too early to look for with any confidence in later years." The pattern he sees is an effort by Americans "to restore, with only minor changes, the open world of which the Enlightenment had dreamed in the eighteenth century and the Liberals in the nineteenth" (p. xi). It was a world that was originally to have included the Russians, but when that proved impossible Washington devoted its energies with greater success to reconstructing the international order within the narrower sphere of influence open to it. Consequently there emerged two competing systems of political, economic, and social organization that by the end of 1962, the date at which Balfour chooses to close his account, had begun to show signs of wary mutual forbearance.

This is not an implausible interpretation, but it is hardly an original one either, given all that has been published on the Cold War during the past two decades. Balfour has not gone on to explore the kinds of questions that preoccupy Cold War scholars today: To what extent were the respective superpower systems compatible with the interests of other countries? Did Russians and Americans expand their systems over the objections of third parties or at their request? What internal influences shaped the external character of these systems? What role did misperception play in determining the course of events? Nor has Balfour attempted to exploit the archival source material on the Cold War now abundantly available on both sides of the Atlantic.

The strong point of the book is its coverage of European economic developments. The Marshall and Schuman Plans, the West German "economic miracle," and the European Economic Community get more attention than is usually accorded them in surveys of this kind: it is obvious that this is where Balfour's main interest lies. But even here his discussion tends to focus more on structure and organization than on functional significance; it is also, at times, poorly integrated with the rest of the book. A concluding speculation on the desirability of economic growth seems especially out of place—as Balfour himself warns us in his preface (p. xii).

To his credit, Balfour acknowledges the uneven character of his book. "It is usually prudent," he writes, "to avoid discussing matters about which one is ignorant" (p. xiii). Maybe so, but authors of interpretive surveys, of necessity, do it all the time. Balfour's reluctance to generalize, speculate, and provoke (especially his refusal to deal at all with

events since the Cuban missile crisis, where such writing is most needed) will limit the usefulness of this solid but bland book.

JOHN LEWIS GADDIS
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PAUL B. RYAN. *First Line of Defense: The U.S. Navy since 1945*. (Hoover Press Publication, number 237.) Stanford: Hoover Institution Press. 1981. Pp. xvi, 224. \$14.95.

If retired navy captain Paul B. Ryan's new study of the U.S. Navy since World War II provides any criterion, then Georges Clemenceau's notion about war being too important to be left to the generals should be modified to suggest that naval history is too important to be left to former naval officers. Perhaps it all started with Alfred Thayer Mahan's adroit use of maritime history to lobby for a new naval policy and fleet rebuilding in the late nineteenth century. If so, then Ryan learned his mentor's lessons well—a tribute to the past century of naval war college training of its senior professionals. His history borders on policy science; his tone almost propagandistic. Yet, the story of the navy in our era seems almost too ripe for proper historical interpretation to be left merely to rearmament zealots now emerging around the country.

Modern America has wrestled continually with one abiding strategic problem. Should we be a continental or a maritime power? From Mahan to Ryan, navalists have pushed for the latter, while events of the Cold War and even detente have consistently lured the country elsewhere in Europe, Asia, and the Middle East. It should come as something of a shock to find even so knowledgeable a naval-maritime historian as Clark G. Reynolds proclaiming in his introduction to Ryan's work that as the century wanes, the U.S. Navy still remains this nation's "first-line of defense" (p. ix). Surely we all know that a triumvirate of land, sea, and air power forms the first line of military defense in the nuclear age, and this review seems hardly the place to remind the citizenry that diplomacy, not the military, really provides such a line for all nations in all ages. But, Ryan, Reynolds, and the Hoover Institution Press apparently view this book as the proper place for interjecting the historical craft into current bleating from Washington about the sad state of our defense. There is an obvious danger in working with contemporary history, and whatever the value of Ryan's Mahanite–Theodore Rooseveltian use of the discipline, we must remain wary of its pitfalls.

Ryan employs expert techniques in using statistics, diagrams, charts, and photographs to enhance the text of a well-paced narrative. The reiteration of the navy's story from the halcyon days of World

War II to its somber decline under one of its very own as president, has been neatly packaged and highlights what Ryan takes to be the major issues. To him it is a story of decline, neglect, and the changing nature of American sea power. From the unification battles immediately after the war to what Ryan calls "America's Great Turnabout" (p. 167) at present, the topics receive succinct coverage. Once more we learn about an institution philosophically geared to win but forced to tailor its power to limits prescribed by civilians. The professional questions of roles and missions, ethics and duty, drugs and desertion come forth much as they have from media analysis over the past several decades. The author provides good treatment of Pentagon–Capitol Hill budgetary infighting and captures the major internal hassles of naval airpower "jocks" (carrier admirals) versus Hyman Rickover's nuclear mavericks who carry even more clout in Congress. He seems almost too strident in portraying innocent sailors as victims of the "whiz-kid" militarized civilians from Robert McNamara to Harold Brown. How officers from all three services squirm with memories of McNamara's tenure in the Pentagon, and even Annapolis graduate, and later Commander-in-Chief, James Earl Carter comes off with scant praise from Ryan. Civilians generally fare poorly in this book, and even the emasculated service secretaries (John Warner and John Chaffee for instance) get lost as the author tries to cover too much too quickly. Had Ryan substituted fuller treatment for his rather constant editorializing, the reader might have gained deeper insight into the real problems of the postwar navy. As it is we learn far more than we really need to about the father of the Soviet naval threat, Sergei G. Gorshkov, and too little about the role of modern American "captains of industry" from General Dynamics and Litton Industries in helping to shape the direction of the U.S. Navy.

To characterize Admiral "Bud" Zumwalt as a Teddy Roosevelt Progressive (p. 68) may strain credulity, however praiseworthy his contributions to internal naval reform. Similarly, to dismiss Korea as a "rehearsal" for Vietnam (p. 16) does disservice to the facts and personalities of both episodes. Then too, Ryan confuses the professional term "joint" with "combined" (p. 74) when discussing allied exercises. This seems typical of an American military generation that is convinced of its own hegemony and has constantly slighted NATO naval power in all its comparisons with Soviet capacities.

All of this is simply to cavil with Ryan's particular and parochial tone that, however, still governs far too much writing on military and naval affairs. True, most original records of real value remain closed to scholarly research because of security rules (notwithstanding the Freedom of Information Act

and the admirable declassification program of the National Archives). So Ryan and other students of contemporary military-naval history must fall back upon decidedly slanted but accessible documentation for their work. *First Line of Defense* sets forth major issues facing one military institution in our era. It gives both the public and the profession a tantalizing taste of a rich topic awaiting more thorough interpretation.

What a wonderfully missed opportunity for Ryan to have dispassionately contributed a comparative analysis of the present era with that other great period of American naval decline and renaissance after the Civil War. Similarities abound including the issues of obsolete weapons systems, technological revolution, civil-military conflicts about budget, policy, industrial relations, roles and missions, manpower procurement—even the *raison d'être* for the Marines! The eternal squabbles between barnacle-encrusted senior officers and the avant garde of the “new” professionalism can be found in the two periods. Perhaps the threat perceptions will seem different, but one need only substitute the word Soviet for British, German, or Japanese in the documents of the two eras. True, the war with Spain hardly equates those with Korea and Vietnam, especially as the navy played such an ancillary role in those later conflicts. But, if the focus is on the first line of defense, deterrence through superior martial might, and American military-moral rebirth, then the comparative historical study seems worthwhile. Instead, Ryan has given us a 200-page tract on our own age that seems much more like a Pentagon or Reagan position paper.

BENJAMIN FRANKLIN COOLING
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THOMAS R. DUNLAP. *DDT: Scientists, Citizens, and Public Policy*. Princeton: Princeton University Press. 1981. Pp. 318. \$18.50.

In this fine contribution to American environmental history, Thomas R. Dunlap treats natural science as a proper subject for social historians, not merely as old science and a thing apart, suitable only for investigation by scientists nearing retirement or by philosophers of science interested in epistemology. Dunlap believes the explanation of developments in the history of DDT lay not exclusively in the internal components of related scientific disciplines but also in institutional history, economic arrangements, personal ideological alignments, and just plain self-interest. The early commitment of entomologists to pesticides instead of general biology and scientific ecology was more the result of society's demand—especially the agricultural sector's—for a quick, clear victory in a war against bugs than it was the result of internal research needs and the internal momentum

of the relevant sciences. Consequently, wildlife biologists, not chemists or entomologists, first became alarmed about the possibility of harmful side effects from the use of DDT. The author of a popular book, Rachel Carson, and a handful of lawyers who formed the Environmental Defense Fund stirred up public concern that led eventually to a change in the direction of research toward the study of the general environmental impact of DDT. This is not to say that Dunlap ignores the scientific side of DDT's history. A vital part of the story is the clash of scientific opinion, which he delineates lucidly.

The first part of the book sketches the background of pesticide use and regulation, summarizing and updating James Whorton's *Before Silent Spring*. The discovery of DDT and widespread use of the chemical following World War II is then examined. Next, the public reception of Carson's *Silent Spring* and the reaction of scientists is described in a more objective treatment than is available in Frank Graham's *Since Silent Spring*. The Environmental Defense Fund's legal challenge in Wisconsin, in 1968–69, gets two chapters. The final two chapters describe events leading to the ban. The narrative is interrupted at appropriate places to summarize key issues, to generalize broadly from the evidence, and to identify historical patterns.

Perhaps the Wisconsin hearings should have been given less space and, for balance, the reports of the General Accounting Office, the Mrak commission, and the Hilton commission should have been analyzed in more detail, but Dunlap thinks that events in Wisconsin were of pivotal importance and established a pattern for later debate. The ban did not end the debate, and as it proceeds this first-rate case history should be useful homework for policy makers and concerned citizens, as well as students of recent American history.

MORGAN SHERWOOD
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TINSLEY E. YARBROUGH. *Judge Frank Johnson and Human Rights in Alabama*. University: University of Alabama Press. 1981. Pp. ix, 270. \$19.95.

As the preface indicates, this book is a history of the human rights movement in Alabama, 1955–79, and is written appropriately in the form of a biography of Judge Frank Johnson whose twenty-five years as a United States district judge ended with his appointment to the Fifth Circuit Court. Tinsley E. Yarbrough has traced the movement to end segregation and racial discrimination in the state, but he covers as well reapportionment of the rural-dominated legislature and Johnson's controversial decisions ordering improvement of Alabama's mental health and penal systems and of its inequitable

property tax system. These decisions are discussed in a sympathetic manner, although Yarbrough indicates some concern about excesses of judicial power. He finds, nevertheless, that Judge Johnson was one of the few authentic heroes produced in the state during a troubled period.

Several years ago Abram Chayes suggested in a law review article that since 1875 the courts have moved from a private law model to a public law model. The latter includes such elements as determination of the scope of a case by both judge and the parties to it, a sprawling structure of parties to the action, flexible continuing relief rather than a one-shot decree, and an active participating judge who is more than a passive arbiter. The public law model thus balances competing policy interests in specific situations. A judge who follows it is more likely than not to be termed a judicial activist and to evoke, as Johnson did, threats, vitriolic abuse, and even violence.

Such reactions to Johnson's decisions might have been less acute had not his principal antagonist been George C. Wallace, a one-time law school classmate who was governor of Alabama twelve of the years covered in this study and his wife's chief adviser for her two years in office. The dramaturgy of the Johnson-Wallace conflict tends to overshadow the issue of federal judicial control of state government, which Yarbrough recognizes in chapter 10, "The Real Governor of Alabama." When alleged states rights collided with those of the poor, the black, the mentally ill, the imprisoned, and other disadvantaged, there was little doubt where Johnson's sympathies were. In a sense, his reading of the Constitution moved Alabama governments toward a more open, free, and representative system.

Yarbrough has documented his study carefully, although it is curious that no interviews with George Wallace and several important mental health officials are listed in the bibliography. This book should stimulate study of the human rights movement in other states as well as its history in other federal district courts, an area in which considerable research remains to be done.

ROBERT B. HIGSAW
University of Alabama

WILLIAM H. CHAFE. *Civilities and Civil Rights: Greensboro, North Carolina, and the Black Struggle for Freedom*. New York: Oxford University Press. 1980. Pp. xii, 436. Cloth \$13.95, paper \$5.95.

To most observers of the post-1954 American South, white supremacy is generally associated with the strident rhetoric of massive resistance, with the absurd posturing of yahoo legislators, with law men wielding cattle prods and fire hoses, and governors who shamelessly pandered to the cracker vote by

"standing in the schoolhouse door." There were, however, more subtle and, indeed, more effective ways to resist social innovation. "Various Southern communities," as William H. Chafe observes, "had different ways of saying 'Never'" (p. 170). Little Rock, Arkansas, and Philadelphia, Mississippi, said it in the crude accents of anarchy and violence. Greensboro said it softly, even courteously. With the notable exception of a police and national guard riot on the campus of North Carolina Agricultural and Technical College in 1969—a tragedy that, as Chafe plausibly suggests, may have been incited by FBI agents provocateurs—it managed to deny equality to blacks and yet retain its reputation as an enlightened city of the New South. In this engrossing and well-written study of social change in an affluent and urbane community ("governed by sophisticated lawyers associated with large corporations" [p. 6]), Chafe puts to rest the comfortable notion that, but for the benighted redneck, the better class of whites would have done right by the Negro.

On the surface, Greensboro was an unlikely setting for racial struggle. The first Southern city to announce its intention to comply with the Supreme Court's school desegregation decree, its image was that of a "beacon of Southern progressivism" (p. 6). Yet from 1960, when four youths began a sit-in at Woolworth's lunch counter to launch the Southern student movement, to 1971, when dilatory community officials finally acceded to court-ordered school desegregation, the city was host to a series of polarizing confrontations between blacks who sought only justice and a "moderate" white power structure that scrupulously honored the forms of "good race relations" but denied the substance of human rights. Through it all, Chafe writes, Greensboro managed to preserve the status quo ante—Brown by wrapping itself in a sweet-tempered intransigence, in a "progressive mystique" that defused dissent with grace and gradualism and professed openness to new ideas even as it hewed to old customs. In the paradoxical tradition of North Carolina—the state that V. O. Key praised for its "progressive outlook" and that twice elected Jessie Helms to the United States Senate—Greensboro's civic and business leaders shrewdly avoided the harsh language of massive resistance but gave ground only before irresistible pressure and then only enough to contain black protest. "We're just like Georgia and Alabama," one of Chafe's sources noted, "except we do it in a tuxedo" (p. 42). Or as another interviewee said, Greensboro was a "nice-nasty town" that indulged itself freely in the politics of good intentions—the better to pre-empt the forces of change.

Chafe's work, then, is local history at its most suggestive. It provides a meticulous narrative of black protest and white resistance in one city during thirty years (1945–75) of ferment and change. It

demonstrates anew that the black struggles of the 1960s were deeply rooted in a tradition of black pride and protest. It reveals the processes whereby youthful insurgents, confronted by unyielding white opposition, lost faith in "the system" and turned to separate community development and the slogans of black power in their search for self-definition and self-determination. It offers disturbing new evidence of the depths to which the FBI stooped to discredit black activism and to provoke the irrational use of white force. But it also examines the "sham of moderation," the means whereby a sophisticated power elite can regulate conflict and preserve a semblance of social harmony in the face of a determined and remarkably cohesive opposition. Civil rights and civility, as Chafe concludes, are not necessarily compatible. In fact, in a "context of oppression," civility merely "provides a veneer for more oppression" by those who would "guard power under the guise of sharing it" (p. 249). The basic argument is not novel; revisionist social scientists and historians have already begun to explore its class dimensions. But no scholar has so systematically used it in an analysis of racial confrontation. Viewing Greensboro as a microcosm of the nation, Chafe has boldly suggested an interpretive framework in which to examine the larger struggle for Afro-American rights.

Chafe is codirector of the Duke University oral history program, and his note on sources suggests that his work benefits richly from some seventy-five interviews (the "indispensable core of this book") in judicious combination with a wealth of manuscript and printed sources. Although the endnotes are hopelessly abridged in the paperback copy supplied to this reviewer, the hardback edition, or so the author avers, is "more complete" in its documentation.

NEIL R. MCMILLEN
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PETER IVERSON. *The Navajo Nation*. (Contributions in Ethnic Studies, number 3.) Westport, Conn.: Greenwood Press. 1981. Pp. xxxii, 273. \$25.00.

During the last fifteen years, the Navajo tribe has been the subject of intensive study by scholars in history and anthropology. Half a dozen, excellent, book-length studies have appeared, all of which have added greatly to our understanding of Navajo history. Now to this growing list of key works can be appended Peter Iverson's *The Navajo Nation*. Iverson, assistant professor at the University of Wyoming, has made a significant contribution to ethnohistory as he relates the story of recent Navajo political history from an Indian or tribal perspective.

Today, the Navajos are the largest Indian tribe in the United States. The Diné number some one

hundred seventy-five thousand individuals and reside on a sprawling reservation that covers an area roughly half the size of the state of Ohio. Although many Navajos are poor, living in conditions that can only be described as squalor, the tribe as a unit receives a substantial income from mineral and oil leases. More than most Indian tribes, the Navajos learned to adapt to the white man's world and became acquainted with the white man's ways of doing business. Throughout his book, Iverson points out examples of the Navajos altering their former ways in order to succeed in an ever-changing environment. Consequently, a principal theme found throughout this work is that of a people able to retain their sense of Indian identity, while still making profound cultural adjustments. Other tribes, far more traditional than the Navajos in regard to retaining "traditional" values, have been far less successful at dealing with the cultural and political realities of the present world.

The Navajo Nation presents the history of the Diné in a straightforward chronological approach. The first three chapters of the book treat Navajo history prior to the 1960s. For scholars of the Navajo tribe, this section of Iverson's book offers little in the way of original research. But those readers without expertise in Navajo studies will find them to be a concise, valuable distillation of the latest research on the tribe. Moreover, without these background chapters, Iverson's original research on recent Navajo politics would lack an adequate historical perspective.

Iverson's thesis in the *Navajo Nation* is that the Navajos believe themselves at present to constitute a nation; not in a jargonistic, self-puffery sense, but in a real, legal sense. What many Navajos hope is that their nation will ultimately have a quasi-independent status in some areas (not actual foreign nation status) with perhaps more sovereignty than state governments currently possess. The author argues provocatively that this has been a recognizable trend of Navajo history since the 1860s. At that time the United States government created tribal government for the Navajos as an administrative convenience for the federal government. In a short time, Indian allegiance was transferred from clan to tribe as the Diné recognized the value and strength that came from unity. Over the years, incidents such as the New Deal's livestock reduction program served to alienate the Navajos from the federal government and at the same time increase their sense of tribal unity.

Without doubt, the best chapters in *The Navajo Nation* are the ones dealing with recent Navajo politics. The reader cannot help but notice the parallels with United States politics. The election for tribal chairman is almost identical in style, practice, and campaign rhetoric to an election of a small

state's governor. The best practitioner of Navajo politics in recent years has been Chairman Peter MacDonald. Iverson admires MacDonald and sees the tribal chairman as a leader who is moving the Navajo people toward the goal of nationhood.

My criticisms of this book are few. In the chapters that treat Peter MacDonald and recent Navajo politics, Iverson has relied almost entirely on Navajo sources, such as the *Navajo Times* and interviews with Navajo politicians. Indian sources, like white sources, need to be evaluated carefully for bias. Second, because Iverson has consciously constructed an inside view of Navajo politics, a concluding chapter that puts the Navajo experience into a broader national perspective would have been appreciated. Finally, at times Iverson's study of MacDonald borders on hagiography, and many Navajos who disagree with MacDonald's politics will disagree with this portrayal.

Make no mistake about it, however. This is a path-breaking book in ethnohistory, and Iverson will be justly complimented for this study.

GERALD THOMPSON
University of Toledo

ROBERT F. FUTREL and MARTIN BLUMENSON. *The Advisory Years to 1965*. (The United States Air Force in Southeast Asia.) Washington: Government Printing Office. 1981. Pp. xiii, 398.

This volume is a welcome addition to the literature on the evolution of American military involvement in what is commonly called the Vietnam War. (The "Second Indochinese War" is a more accurate term, but the naming of conflicts is not done by historians.) The title of the book is to the point: a study of the role of the United States Air Force in Southeast Asia during the "advisory years" to 1965. The period covered starts after the conclusion of the Second World War in 1945, but the emphasis quite rightly is on the Kennedy years (pp. 63–192) and the Johnson years (pp. 195–268). Especially commendable are the appendix focusing on the growth of major American and Vietnamese air force units up to February 1965; the maps, charts, and graphs; and the glossary of terms, many of which are not familiar to the layman. The notes to the chapters are largely primary sources, but the bibliography relative to nongovernmental references could be improved.

The volume considers matters as they relate to the air force, for example, how best to advise an ally and in particular one like Vietnam, the role of air power when used in an insurgency and especially in a jungle environment like Vietnam, and the complex issues of coordination with other American military and civilian agencies involved in a common effort. Given the important advisory role of the American

air force to the Vietnamese air force especially after 1961 and into 1965, the book has much to relate. At the same time the study is more descriptive than analytical; the facts are well presented—often allowed to speak for themselves. As America's experience in Vietnam produced a literature of controversy and detoured the course of Southeast Asian studies in the United States for at least a decade, it is time for more matter-of-fact volumes on Vietnam. Yet descriptions, not to mention evaluations, of what happened can be controversial. There are chapters in the volume under review on Dien Bien Phu, the fall of the Diem government, and the Gulf of Tonkin incident, which involved three presidents of the United States in controversial decisions at the time and perhaps in retrospect. The careful presentation of the role of the United States Air Force in Southeast Asia during the "advisory years" in Vietnam is the lasting contribution of the study. Further books in the series will focus on the American air force in South Vietnam and Laos and over North Vietnam up to the end of its "direct role," which came in 1973.

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CANADA

CHARLES M. JOHNSTON. *McMaster University*. Volume 2, *The Early Years in Hamilton, 1930–1957*. Buffalo: University of Toronto Press. 1981. Pp. xvi, 330. \$25.00.

STANLEY BRICE FROST. *McGill University: For the Advancement of Learning*. Volume 1, 1801–1895. Montreal: McGill-Queen's University Press. 1980. Pp. xvi, 313. \$25.00.

In Canada two types of university histories have recently become popular. Reminiscences such as *Halfway up Parnassus: A Personal Account of the University of Toronto, 1932–1971*, by Claude Bissell, and *The McGill You Knew: An Anthology of Memories, 1920–1960*, edited by Edgar Andrew Collard, capture the essence of a particular university; formal studies like the two books reviewed here preserve the origins and struggles of an institution in greater detail.

Although Charles M. Johnston's book is the second in a series, his prologue adequately summarizes the findings of the earlier volume. This book begins with the campaign to move McMaster University, which was founded in 1887, from Toronto to Hamilton, Ontario, because a new location would serve as "either a cure for every university ailment or a springboard to new and exciting opportunities" (p. 31).

Johnston deals with the growth of the school into a modern university and emphasizes the importance of the natural sciences. Although he notes that the withdrawal of the Baptist Convention from involvement in the affairs of the university (1957) paved the way for McMaster to become a "front rank" university (p. xii), he does not fully explain why that religious affiliation had been prohibitive. Ultimately the move from Toronto as well as the break with the Baptist Convention allowed McMaster to develop as a major university because people no longer compared the school to or confused it with the University of Toronto.

Johnston emphasizes the conflict in every decision and writes at length about disputes between the chancellor and professors, rifts between student groups, and resistance to change. Although selective editing would have resulted in a more concise and readable book, this encyclopedic study represents massive research that will be helpful to future scholars.

Stanley Brice Frost's book, a livelier history, emphasizes the personalities involved in creating McGill University. Although McGill predated McMaster by more than half a century, there were many similarities in their origins. Both schools were established at the bequest of successful businessmen. Both schools had strong religious affiliations (McMaster with Baptists, McGill with Anglicans), both grew with private support, and both assigned science an important role in the curriculum. Ultimately both universities deviated from what their founders had intended and developed a new form of institution appropriate to Canadian needs.

Frost, a former vice-principal of McGill University, begins his study with a brief history of the British colonies. James McGill, a merchant, political leader, magistrate, colonel, and philanthropist, endowed a college due to "a measure of public spirit" and a search for "some small piece of immortality" (p. 19). Many early officials at McGill were influential in its growth, but one of the most dynamic leaders was the Canadian-born John William Dawson, who served as principal from 1855 to 1893. Dawson, a deeply religious man, was a noted scientist and educator who believed in making education available to as many people as possible. He supervised the growth of the library, the collections of geology and natural history, and in 1884 instituted "equal but separate" (p. 253) education for women. Frost credits Dawson with bringing McGill from near bankruptcy to solvency and with making a university out of McGill College.

Frost details the formation of the various departments and includes a separate chapter on the well-known Faculty of Medicine, which predated McGill University. Founded in 1815 as a house of recovery for immigrants of the Napoleonic wars, the medical

institution became associated with McGill in 1832, thereby enabling McGill—which until then existed only on paper—to claim active engagement in education.

Frost, who is now writing a second volume, presents his carefully researched material with enthusiasm. The book, however, suffers when he leaves his sphere of expertise to offer a brief history of Montreal's financial situation in the 1850s, to write a synopsis of women's colleges in the United States, or to assess Dawson as a scientist. Furthermore, in a book intended for an English-speaking audience the French quotations are distracting. Nonetheless, this book, which contains many illustrations of McGill and the people who influenced its growth, is a valuable aid to those who seek knowledge about McGill University specifically, or Canadian educational history generally.

KAREN C. ALTFEST

New York, New York

LATIN AMERICA

ARTHUR MACEWAN. *Revolution and Economic Development in Cuba*. New York: St. Martin's Press. 1981. Pp. xvi, 265. \$22.50.

Arthur MacEwan argues that the Cuban Revolution has cleared the way for economic progress by destroying the old structures that had held the country in a condition of underdevelopment. He also notes, however, that the successes themselves have often created new problems. For example, the reduction of overt long-term as well as seasonal unemployment—a social success—reduced the incentives for especially hard work during the harvest season, thus contributing to economic setbacks. MacEwan focuses on Cuba's agriculture because of its important role in the country's past and present. He discusses briefly the role of agriculture in pre-revolutionary Cuba but spends over half the book on the period from 1959 to 1970. The book is also a self-consciously Marxist attempt to analyze contemporary Cuban affairs.

The book has several merits. It extends the work on Cuban agriculture from a Marxist perspective beyond James O'Connor's *The Origins of Socialism in Cuba* (1970). It incorporates Brian Pollitt's seminal, but difficult to locate, work on Cuban agriculture and the contradiction between social success and economic setbacks engendered by unemployment reduction. At times it includes frank discussions of Cuba's problems and difficulties from a Marxist viewpoint. The work also analyzes thoughtfully the lasting structural problems in the organization of work in Cuba, describing them as the major sources of economic difficulties.

The book also has some problems. Although published in 1981, internal evidence suggests no substantive changes have been made since early 1978. MacEwan adds nothing to the understanding of Cuba's severe economic crisis of 1979–80. His arguments about Cuba having "considerable success in overcoming the worst symptoms of disorganization" (p. 205) in the 1970s run afoul of the available evidence. His belief that the government would continue to move toward a reduction of private sales by farmers (p. 197) is factually wrong. At times his contingency analysis is right but his "gut" forecasts are wrong. For example, he argues that worker alienation will rise as a consequence of rising managerial authority in the enterprise unless there are effective labor organizations. He suggests Cuba solved this problem in the 1970s (p. 185). The evidence suggests it has not; MacEwan proves analytically prescient but empirically wrong.

There are also serious omissions. MacEwan makes no use of works that would often help his argument (those of Lourdes Casal, Max Azicri, or Marifeli Perez-Stable). He is simply unwilling to accept evidence of prerevolutionary economic growth, asserting boldly that simple stagnation occurred. He is uncritical of the prerevolutionary Communist party, suggesting a persistent "progressive" position without ever mentioning its long collaboration with Fulgencio Batista's dictatorship or its flirting with Gerardo Machado's dictatorship (pp. 24–26).

MacEwan chides critics of the Cuban experience who lack a "theory of socialist development" (p. 123); MacEwan arguably lacks a "theory of socialist decay." Although "labor unions during the 1960s increasingly became instruments of the state rather than organizations of the workers," playing "an ever-decreasing role in the organization of production" (p. 152), and despite his statement that "the leadership was becoming separated from the masses of the people," MacEwan never considers whether Cuba stopped being "socialist" by his standards. He could also have discussed (which he did not) the increased use of coercion in the late 1960s.

The main weaknesses of MacEwan's book are thus two: insufficient theoretical reflection on Cuba's problems of the 1960s and insufficient empirical assessment of Cuba's performance in the 1970s.

JORGE I. DOMINGUEZ
Harvard University

PAUL J. VANDERWOOD. *Disorder and Progress: Bandits, Police, and Mexican Development*. Lincoln: University of Nebraska Press. 1981. Pp. xix, 264. Cloth \$21.50, paper \$8.95.

Paul J. Vanderwood has researched and published extensively on Mexico's famed rural police force,

the *rurales*. The author's current work is an expansion and extension of his earlier studies, with the *rurales* still playing a prominent role. Using what he classifies as a "bottom-up" focus, Vanderwood traces the changing interrelationships of order, disorder, and development in Mexico from the colonial period to the early twentieth century. The theoretical framework depends heavily on the writings of Eric Hobsbawm on banditry and David Bayley on the political foundations of modern police forces, and there are frequent comparisons with developments in other countries, especially Europe and Latin America.

Although *Disorder and Progress* involves a survey of over three centuries, most of the attention is focused on the period from 1857 to 1911. Beginning in 1857, Mexican Liberals attempted to introduce a program of republicanism, capitalism, and individualism that placed them at odds with the Catholic church and the military and that led to civil war and ultimately foreign intervention in the 1860s. Banditry had been endemic since the earliest days of independence, but the continuous turmoil of the decade from 1857 to 1867 conferred on bandits a political power that they had not previously exercised. Bandits displayed little interest in the political issues involved, being primarily concerned with enhancing their own political and economic fortunes. To cope with the chaotic situation, the Liberal government formed a rural police force; although the Liberals were nominally committed to federalism, Vanderwood views the rural police as essentially a tool for political centralization. This rural police force endured a sputtering start, but by 1875 there were more than one thousand *rurales* in service.

The forces of order, disorder, and development underwent major changes with the rise to power of Porfirio Díaz, who dominated Mexican politics from 1876 to 1911. Firmly dedicated to the promotion of economic development, Díaz realized that at least a minimal level of order must be maintained if capitalists—especially foreign capitalists—were to invest in Mexico. In the achievement of domestic order, Díaz viewed the *rurales* as his personal agents and often employed them for political purposes rather than strictly for law enforcement. In particular, the long-time president of Mexico (1877–80, 1884–1911) went out of his way to build up the image of the *rurales* as a ruthless and efficient constabulary that could outride and outshoot any bandit group. Vanderwood maintains that there was often little substance behind this image. The *rurales* had major problems with corruption, desertion, turnover, alcoholism, and equipment shortages; instead of being recruited from bandit gangs as the popular image maintained, the *rurales* came mainly from the ranks of artisans and peasants. Many of the *rurales* were

poor riders and worse marksmen; during an annual parade in Mexico City, the chief of the *rurales* fell off his horse. Despite these difficulties, the author concludes that "the Rurales were obviously doing a good political job for Porfirio Díaz, and that had always been their main assignment" (p. 118).

When the Revolution of 1910 erupted, neither the army nor the *rurales* were able to save the regime of Porfirio Díaz. The *rurales* staggered through the early years of the revolution and were even scheduled for a major expansion by Díaz's successor, Francisco Madero, who was searching for some type of employment for his now unemployed revolutionary supporters. A later effort to reorganize and reform the *rurales* was unsuccessful, and Mexico's famed rural police force ultimately became a victim of the very disorder that it had been created to suppress.

Vanderwood has done extensive research in a variety of Mexican archives for his study, and his theoretical framework and comparisons provide the work with useful reference points. *Disorder and Progress* is primarily concerned with bandits and police rather than Mexican development and is exclusively rural in its orientation. With these modest limitations, the work will be of interest not only to students of Mexican history but also to those concerned with the development of national police forces and their roles in political life.

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PETER V. N. HENDERSON. *Félix Díaz, the Porfirians, and the Mexican Revolution*. Lincoln: University of Nebraska Press. 1981. Pp. xi, 239. \$18.50.

Judged by how most historians choose topics, Peter V. N. Henderson is something of an unorthodox scholar, for his book is about an unmitigated failure who, to add insult to injury, was an opportunist and an inept schemer but, oddly enough, was born with a silver spoon in his mouth. His name was Félix Díaz, and he was the son of General Félix Díaz, Sr., a hero of the wars against the French and their puppet Maximilian and, more important, the brother of don Porfirio, who ruled Mexico with an iron hand until 1911. Félix Díaz, Jr., died in 1945 at the age of seventy-seven, undoubtedly thinking his life a failure.

Yet, as Henderson tells the story, the nephew of don Porfirio led a singular life. Brought up at a time when his family reigned supreme in Mexico, he had early accumulated a sizable fortune, probably through his ties with friends in high places. His marriage to Isabel Alcolea survived half a century of political ups and downs. Like his father and his uncle, he reached the pinnacle of a military career, winning the rank of general in the old Porfirista

army. In 1902 he nearly became governor of the state of Oaxaca, a post held by both his father and his uncle. He was loyal to his friends and to the cause of Porfirismo, even when it was no longer a realistic option in the days of revolutionary rhetoric after don Porfirio had gone off to exile in France. Between 1916 and 1920 Félix Díaz, Jr., headed the effort of the old guard to turn back the clock, personally leading military campaigns against the new caudillos of Mexico. To win public support he even turned reformer, offering to bring about a modicum of land reform and racial justice. Still he failed, as Henderson concludes, for lack of charisma, poor military skills, and the absence of political acumen. He "always struck with cold irons."

Henderson's book concerns the Porfirians and the Mexican Revolution, as the title states, and portrays a formerly privileged elite that had outlived its usefulness. To write his story Henderson labored in the archives; used published documents, memoirs, and contemporary accounts; and read with care secondary works. Because he did this with diligence and enthusiasm, he brings to life the saga of a man who until now has been only a name in these crucial years of Mexican history.

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CYNTHIA MCCLINTOCK. *Peasant Cooperatives and Political Change in Peru*. Princeton: Princeton University Press. 1981. Pp. xvii, 418. Cloth \$27.50, paper \$6.95.

"We must forge a new political consciousness in Peru . . . a new society for a new man." With these words of former Peruvian president Juan Velasco Alvarado, Cynthia McClintock begins a searching analysis of the Peruvian military's attempts to reform or revolutionize the structure of Peruvian land tenure in the period 1968–77.

All the great social revolutions of the twentieth century (for example, those of Russia, China, and Cuba) had as a primary goal the destruction of the cultural norms of the *ancien régime* and the substitution of new value systems that would result in the creation of a "new man." In Peru the vehicle for this change was the "self-management" enterprise. As McClintock notes, however, Peru, with its traditional patron-peasant structure still largely intact in 1969, seemed to be a less likely place for change than Cuba or China. Thus she sets out to discover if a self-management strategy did indeed result in changing peasant values and, if so, "what kind of new values would appear" (p. 16).

In contrast to similar studies of Cuba, China, and Mexico, where scholarly access was either difficult or impossible and where little or no prereform baseline

data were available, Peru offered near perfect conditions. Access was no problem and superb pre-reform data existed in the 1969 Cornell-Instituto de Estudios Peruanos study of numerous haciendas and peasant communities. For each area McClintock selected three peasant cooperatives (two on the north coast and one in the highlands) and two control villages that had not been incorporated into cooperatives.

After a cogent description of the Peruvian agrarian structure and peasant political culture before 1969, the author moves to a detailed analysis of the impact of self-management on the attitudes and actions of the peasants. She convincingly demonstrates that there were substantial alterations in peasant attitudes regarding political participation, leadership, social behavior (for example, trust and friendship), and, to a lesser extent, collaborative work behavior. In so doing she effectively shows that the program neither fit the corporate model nor achieved Velasco's goal of a "fully participatory social democracy."

In part 3, McClintock discusses the self-managed cooperatives within the national economy and polity and concludes that here the government suffered its greatest setback. Since the government-sponsored peasant federations "failed to provide sufficient incentives for solidarity among Peruvian peasants . . . the cooperatives remained islands in stormy economic and political seas, and members unconvinced of the potential value of aggregating peasant political demands or of integrating peasant political demands or of integrating economically" (pp. 284–85). Moreover, since the government was never able to overcome traditional peasant skepticism toward the national government, "members turned their new political solidarity into a weapon against what they considered government encroachment on 'their' self-managed cooperatives" (pp. 313–14).

In the final section the author compares the successes and failures of the self-management experiment in Peru to those in Allende's Chile, Cárdenas's Mexico, and Tito's Yugoslavia and asks the question "is self-management a dangerous policy for a Third World government whose authority may not be fully established?" (pp. 342–43). She responds that the danger "to the Velasco, Allende, and Cárdenas regimes was not the fault of self-management per se, but was rather a reflection of the inability of these governments to follow through with the movement toward participation that they themselves had initiated" (p. 345).

This book is social science at its best and constitutes a masterful monographic study that challenges many previously held tenets about the Peruvian "revolution" in particular and peasants in general. Unlike the hundreds of "analyses," "studies," and

"polemics" about the Peruvian experiment that have appeared since 1968, this book will stand the test of time and will remain the definitive work on the subject for years to come.

THOMAS M. DAVIES, JR.
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MICHAEL GROW. *The Good Neighbor Policy and Authoritarianism in Paraguay: United States Economic Expansion and Great-Power Rivalry in Latin America during World War II*. Lawrence: Regents Press of Kansas. 1981. Pp. xi, 163. \$20.00.

The inaccessibility of British and American archival materials as well as pressing Cold War concerns combined for nearly thirty years after the end of World War II to inhibit critical study of great-power relationships and rivalries in Latin America during the 1930s and 1940s. Clearly, however, there were problems with the received wisdom. The captured German documents, fragmented and inconclusive as they are, have lent little weight to the thesis of grandiose Nazi designs on the Americas; rather the reverse. Suspicions have persisted that Anglo-American relations, particularly in Argentina, were nowhere near as amicable as wartime propaganda would have it—suspicions, too, that such national leaders as Vargas and Perón might have been motivated by something more than reflexive greed and "anti-gringoism." Leftist writers like David Green and Irwin F. Gellman have argued all along that the principal objective of the U.S.'s great antifascist crusade in Latin America was to eliminate German economic competition there. (Not to mention British competition: this latter theme has lately been re-examined to some effect by Ryszard Stemplowski, Mario Rapoport, and most notably C. A. MacDonald.) Well, thirty years have rolled round, much British and American documentation has been released, and the Freedom of Information Act has provided additional possibilities for access to U.S. archives. The result has been an outpouring of new writing on these hoary topics. Much of it is first-rate, and almost all of it makes clear that the shining certainties of wartime will have to be abandoned. Michael Grow's book is a major contribution to this revisionist literature, one with which all historians interested in these questions will have to come to terms.

For this is not merely a book about Paraguay and the war, welcome as that would be by itself. It consists in effect of two linked essays: one a crisply written discussion of U.S.-German economic rivalry in southeastern South America in the 1930s, the other an equally brief narrative of the process whereby Paraguay became a wartime client of the United States. The first is, in my opinion, the more important. Recovery from the Depression, writes

Grow, demanded of both Germany and the U.S. major efforts to increase exports of manufactured goods. These efforts brought them swiftly to a bitter head-to-head rivalry for influence in southeastern South America—southern Brazil, Uruguay, Paraguay, and Argentina—a populous and potentially very prosperous agricultural region with as yet little indigenous industrial capacity. In this competition Germany enjoyed two important advantages: the presence of long-established and highly regarded German immigrant and business communities and the capacity to purchase large amounts of the region's grains, fibers, and meats, commodities that the U.S. itself exported. During the 1930s, moreover, the region's oligarchs were attracted to authoritarian nationalist solutions to social unrest and developmental needs, and after 1939 they showed themselves little inclined to offend the Axis powers, the probable winners of the world war. All this lent a certain shrillness to American denunciations of the Nazi menace to the hemisphere's security and added a certain desperation to the aid programs with which the U.S. sought to wean or bribe the regimes away from nonalignment.

Grow's second essay is a detailed account of the American wooing of presidents Estigarribia (1939–40) and Morínigo (1940–47) of Paraguay. Both presidents transparently played off Germany and the U.S. against each other (a tactic further perfected by their successor, the redoubtably anti-Bolshevik General Stroessner). The price of agreement to U.S. demands for hemispheric solidarity at the Rio Conference of early 1942 was a jerry-built (so to speak) \$7 million aid package—although once it was received Morínigo blithely permitted German business and espionage operations to continue unmolested. But in 1944 the reckoning came: the State Department, by failing to renew assistance programs, began to exert pressure for a democratization of Morínigo's haphazard despotism. The result was what old Latin American hands might have predicted: the savage civil war of 1947. Grow himself maintains that U.S. meddling brought about this unhappy result, thus lending credence to the questionable proposition that democracy was somehow “unnatural” to Paraguay.

Grow, it should be remarked, nowhere demonstrates that his case study of Paraguayan affairs confirms the general propositions in his first essay, for in Paraguay the U.S. moved into what was essentially (and some would say understandably) a power vacuum. Nevertheless his contributions mark a major advance in the serious historiography of power relations in the hemisphere, for which he deserves our gratitude and applause.

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STANLEY E. HILTON. *Hülser's Secret War in South America, 1939–1945: German Military Espionage and Allied Counterespionage in Brazil*. New ed. Baton Rouge: Louisiana State University Press. 1981. Pp. 353. \$20.00.

This solid book exemplifies the new trend in the literature of espionage: scholarliness. Increasingly, books on spies and intelligence are being written by academics instead of journalists. More of them are grounded in the documents, interviews with participants, and other primary sources. More come equipped with a full *apparatus criticus*. They seem to hanker less for the sensational and to press more for accuracy. Overall, the level of credibility has risen.

This trend envelops more than books. Today, unlike ten or fifteen years ago, academic journals print articles on intelligence, universities offer courses in it, conventions hold sessions on it, and professors specialize in it.

All this has largely been made possible by the opening of archives, and Stanley E. Hilton's book is based on an outstanding range of documents. These include not only many surviving German records and American counterintelligence papers but also—most remarkably—the Brazilian interrogations of many of the captured German agents. Hilton, professor of history at Louisiana State University, has used these effectively to give his story immediacy and color and to bring his subjects to life. He tells, for example, of Hans Christian von Kotze, who was captured by the British and “turned”—and then seduced the wife of his controller.

The book recounts the rise and fall of German espionage in Brazil. It describes the recruitment of agents and their organization into rings and details problems of payment and transmission of information (to his credit, Hilton, unlike so many other writers on espionage, does not dodge these technical matters). He tells of the agents' feuds and love affairs, their jurisdictional clashes, and their difficulties in avoiding arrest. The overall picture is one of constant movement, continual busyness—and of results that seem incommensurate with all the effort. Most of the agents' information dealt with the arrival and departure of Allied and neutral freighters and warships. Other reports touched on American war production, U.S. activities in Brazil, and Brazil's foreign policy stance. The rings' lifetimes were measured only in months, however, for under American pressure and with American help Brazil crushed them in 1942 and 1943.

Hilton says that it is “impossible to answer conclusively . . . how useful to the Reich's war effort the information gained . . . was.” This is true, but he has not tried to answer it at all, pleading “archival limitations.” Yet it would certainly have been possi-

ble at least to see whether some of the spies' shipping reports were sent to U-boats and whether they resulted in sinkings, or to see whether any of the political information reached the German foreign minister or Hitler and affected their decisions. Hilton's failure to do this flaws his book and turns it into a tale without an ending.

But the tale itself achieves a rare combination of accuracy and excitement and makes the book a valuable addition to the welcome new shelf of scholarly intelligence studies.

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Great Neck, New York

JOHN W. F. DULLES. *President Castello Branco: Brazilian Reformer*. College Station: Texas A&M University Press. 1980. Pp. ix, 557. \$27.50.

The bottom line in Brazilian political affairs remains the armed forces. John W. F. Dulles's latest volume vividly emphasizes this. For the three years 1964–67 that Castello Branco governed, many structural changes took place in the country, but the role of the military always increased. This book records the changes, infighting, victories, and defeats of President Castello Branco. The civilian politicians were no problem. It was the army that gave the president his greatest headaches, and apparently he narrowly escaped being overturned by "hardline" officers in 1965.

Organized into twelve choppy, chronologically overlapping chapters dealing with political and economic reforms, antigovernment protests, presidential succession, and the electoral contests of November 1966, the material sorely needs a professional historian or editor to give some cohesion and order to this vast and important collection of minutiae, trivia, and rewarding (and often unrewarding) taped interviews of Castello Branco's collaborators and detractors. There is no question, however, that this is an uncritical, laudatory book that praises Castello Branco as an outstanding Brazilian reformer-president. A continuation of a 1978 work by Dulles, *Castello Branco: The Making of a Brazilian President*, which traced the general's life up to 1964, this volume covers his career from April 1964 to his death in a plane crash in July 1967. Twenty-nine pages list the source material, much of it from the Documentary Research Center for Contemporary

Brazilian History, the LBJ archives, and innumerable personal interviews. It should have resulted in a splendid book; instead, this reviewer came away with a deep sense of frustration concerning events in Brazil during this period.

Readers must remind themselves that President Castello Branco came to power by a military coup d'état that based all its legality on its success in having toppled the previous government. Throughout the book Dulles seems to be building a case that Castello Branco's primary interest was to fight for democratic procedures—but always within a military frame of reference. One of the book's most fascinating sections concerns the National Security Law (Decree Law 314). The law, basically still in effect, defines danger to internal security as "employment of propaganda, counter propaganda and actions in the political, economic and psychosocial and military areas designed to influence or provoke opinions, attitudes or behavior of foreign, enemy, neutral or friendly groups against the attainment of national objectives" (p. 448). With this type of vaguely worded law, little new authoritarian legislation is needed to govern Brazil. Most of the political problems faced by Castello Branco still exist in the 1980s. With inflation over 100 percent in 1981, almost as many political parties as existed in 1964, and the economy in disarray, one can honestly ask just what President Castello Branco did reform. The answers, of course, are to be found in the tough economic decisions that he made to set the stage for the "economic miracle" and the emergence of Brazil as an industrializing world power. The Brazil of 1981 is a vastly different country from that of 1964.

Chapter 8, on presidential succession, is one of the best in the book. Dulles explains how Castello Branco tried to let civilian politicians decide who should be his successor, but they refused to accept this freedom and responsibility. Dulles quotes an astute political columnist, Carlos Castello Branco, who wrote that in Brazil power emanates from the armed forces and is exercised in their name. The congressmen were not as politically naive as the reformer-president was. They would not suggest any other name for the presidency but that of the declared candidate, Minister of War General Artur da Costa e Silva, the next president of Brazil.

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Collected Essays

These volumes, recently received in the AHR office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other similar volumes that are amenable to reviewing will be found in the review section.

M.-A. ARNOULD *et al.* *Histoire et méthode*. (Acta Historica Bruxellensia, Travaux de l'Institut d'Histoire de l'Université Libre de Bruxelles, number 4.) Brussels: Éditions de l'Université de Bruxelles. 1981. Pp. 519.

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Communications

A communication will be considered only if it relates to an article or review published in this journal; publication is solely at the editors' discretion. Letters may not exceed seven hundred words for reviews and one thousand words for articles. They should be submitted in duplicate, typed double-spaced with wide margins, and headed "To the Editor."

TO THE EDITOR:

After reading the *AHR's Forum*, "The Two Postwar Eras and the Conditions for Stability in Twentieth-Century Western Europe" by Charles S. Maier (86 [1981]: 327–67), I have decided that the author and his two commentators, Charles P. Kindleberger and Stephen A. Schuker, have failed to define the concept "stability." Throughout there is a mixing-up of (1) economic stability (preferably prosperity) as opposed to either inflation or depression; (2) diplomatic stability—that is, maintaining a favorable balance of power; (3) social stability, or rather the repression by various elites of revolution or reform; (4) ideological stability—that is, conservatives' hostility to liberalism, social democracy, and the Soviet Communist Party; and (5) military stability, or peace rather than war. All three participants in the *Forum* basically address the first issue, economic prosperity, although the four other problems (which hide six or seven additional subthemes) are touched upon.

The footnotes to further literature, especially on the economic diplomacy of the United States toward Western Europe in the two postwar eras, were helpful. But the *Forum* as a whole was not a coherent discussion that really explained why the Peace of Versailles lasted merely twenty-two years, while the Peace of Potsdam has lasted almost thirty-three years, with all the potential of lasting some decades into the future.

The idea of the *Forum* was imaginative, the research was meticulous, but the three participants confused themselves with a cliché—"stability." I be-

lieve the term is just too abstract for historians to write about.

ROBERT H. WHEALEY
Ohio University

PROFESSOR MAIER REPLIES:

I am sorry that the concept of stability seems so muddled to Robert H. Whealey. By stability I mean largely the settled functioning of arrangements to order public life without recourse to coercion, force, or conflict. Stability has many dimensions. If I emphasized economic stability (as measured by high employment and relatively low inflation), that is because in the twentieth century political stability seems easier to maintain when economic stability prevails. Rapid economic growth can itself, however, erode settled political institutions, and not all polities are equally vulnerable to high unemployment (witness Britain in the 1930s).

The other dimensions of stability that Professor Whealey discerns from the *Forum* confuse me. Numbers (2) and (3) seem tangled together. In any case, I sought to show how forces for international and domestic stability interacted, such that the post-Postdam settlement rested on more definitive bases within states and across borders simultaneously. As for Whealey's number (3), social stability, I disagree that it means the repression of reform by elites. As intelligent conservatives recognize, one reforms in order to preserve. Number (4), ideological stability, should not entail hostility to social democracy. I tried to argue that in the late 1940s social democrats seemed to be the fulcrum for political and social stability.

I do not feel that stability is a cliché in the sense of being a hackneyed term. It is hardly a hackneyed subject for historians, although it may be a problematic one. Why historians should not write about abstract terms—such as authority, the state, liberty,

nationalism, revolution, and the like—is not apparent to me; granted, historians must fill out the specifications for each case. Of course, Professor Whealey is welcome to renounce those subjects he finds unfruitful, but not for all of us, I hope.

CHARLES S. MAIER
Harvard University

TO THE EDITOR:

There is no basis for E. I. Brodtkin's statement that my book, *English Historical Writings on the Indian Mutiny, 1857–1859*, was a "rehash of mutiny literature" (*AHR*, 85 [1980]: 700). One wonders if there is any other work of this kind dealing with the genesis of Mutiny studies, with early writers on the Mutiny like Mead, Dodd, and Ball, with the famous historians like Kaye, Malleson, and Forrest, and with various other categories of writers as described in the book under review. The up-to-date bibliography cannot also be ignored. Brodtkin's charge that the books were evaluated only in terms of their closeness to my viewpoint would have been justified had he referred to a single book of importance that was left out, whatever the point of view was.

It was made abundantly clear in the preface and elsewhere (p. 279) that the book was not an attempt to place the revolt in any particular light. That it was a "national war of independence" was never stated as such in connection with my views on the Mutiny. But Professor Brodtkin took a puerile pleasure in stating that nothing short of regarding "1857" as a national war of independence would satisfy me. The reviewer seemed to think that attempt was made to resurrect the "tired dialogue" with R. C. Majumdar, but in fact only a reference to the controversy was made in one page (p. 189) in the interest of historiography.

Professor Brodtkin wrote, "British scholarship is collectively damned after the author sets up some obvious targets and then shoots them down." The allegation is so mischievous that it can be confronted only by reference to my views on the contributions to Mutiny studies of the whole set of eminent British historians: Mead (p. 43), Bell, Norton (p. 14), Red Pamphlet (pp. 46, 223), Russell (p. 12), Cooper (pp. 51–52), Ball (pp. 70–71), Martin (p. 86), Kaye (pp. 111–12), Malleson (pp. 133–34), Forrest (pp. 165–67), Holmes (pp. 167–68), Campbell (pp. 237–38), Gimlete (p. 214), Wood (p. 213), MacMunn (p. 215), MacLagan, Collier, Palmer (pp. 195–98), Hilton (p. 195), and Llewellyn (p. 202), not to speak of other equally important writers like Cave-Brown (pp. 52–53), Prichard, Lowe (p. 63), Dodd (p. 64), Forbes-Mitchell (p. 223), Edwards (p. 37), Hutchinson, Raikes, Gubbins, and others (pp. 60–

61), including American writers like Embree and Metcalfe. If so, how was British scholarship collectively damned?

The other charge is that my attempt to attribute the failure of the movement to the rejection of the Enfield rifle by the sepoys is "simplistic." What I suggested was that British writers in general made no attempt to rationalize the uninterrupted success of British arms in the Mutiny wars, except in terms of racial superiority by extolling the qualities of British national character. It seems that the withering power "of the Enfield rifle which was effective at more than 300 yards distance," as Nicholson reported, in sweeping away the sepoy gunners like chaff, and its killing accuracy as Havelock and North testified, made no impression on them in accounting for the success of the British in weathering the crisis, other factors notwithstanding. Of all of the British writers it was Charles Ball who had the insight to observe, "had the revolted army of Bengal had the rifle in their hands Delhi might still have belonged to the Moghuls. . . . It is impossible to say where the revolt would have stopped had the sepoy been armed with this rifle." Is it "melodrama" or "bad history"? Professor Brodtkin did not have the courage to quote this passage from Ball (p. 277).

The organization of the text, as Brodtkin commented, was arbitrary, circular, and disjointed. No one knows better than the author how difficult it was to organize a work of this type, as I explained on pages two and four. The experts consulted in this matter could not evolve any plan that would be acceptable to all and free from flaws, having regard to the sprawling nature of Mutiny studies with the emphasis on British action and the "vehement self-assertion of the Englishman," which tended to border on the "bizarre," and the regional nature of British military leadership, which acted on its own in a "circular and disjointed" way in the early stages of the sepoy war. Undoubtedly all of these elements or, at least, a portion of them were reflected in the huge corpus of Mutiny literature, which numbers about seven hundred items so far.

S. B. CHAUDHURI
Calcutta, India

PROFESSOR BRODTKIN REPLIES:

I am sorry, but not surprised, that Professor Chaudhuri is unhappy with my review. Had he written a better book, my observations would have been more to his liking. As it stands, he said nothing that is not better said elsewhere. Chaudhuri has served us yesterday's lunch. And it has not been refrigerated.

E. I. BRODTKIN
Connecticut College

TO THE EDITOR:

The October 1981 issue of the *AHR* contains Seymour Becker's very critical review of my book, *Russia and Iran, 1780–1828* (*AHR*, 86 [1981]: 888). At issue is a fundamental disagreement over the nature of Russian expansion in the direction of Iran as well as the question of my scholarly competence. Therefore I feel obliged to respond to a review that so badly misrepresents what I wrote.

Although all of the review's criticisms are unsound, the most objectionable one is the allegation that my interpretation of Russia's motives and actions is colored by an anti-Russian bias. The criteria for determining such bias shift occur mid-review. First the argument is that I am anti-Russian because I see a connection between Russia and Western Europe (regarding motives for expansion). Why it is anti-Russian to argue that leaders in St. Petersburg were guided by motives that would have seemed quite reasonable to Western counterparts utterly escapes me. Later the contention is made that there was a similarity between Russian and Western imperial attitudes in order to show that I have been unfair to the Russians (regarding their failure to understand Iranian and Caucasian points of view). The reviewer's approach seems to be that drawing comparisons between Russia and the West is simultaneously legitimate and illegitimate.

The two examples just cited also reflect the way my arguments have been distorted. In the case of the motives for expansion, the point is made that Russian actions were prompted by the country's own long-established tradition of advancing its borders, not by imitation of the West. This contention ignores the fact that I consider imitation of the West only one of several contributing factors. The chapter where this is discussed states at the outset that "Russian expansion was also a very normal process in terms both of the empire's own historical development and of the diplomatic attitudes of the time. Long before this period, expansion had become a habit of Muscovite statecraft" (p. 22; see also p. 36 *et passim*). The discussion of whether the Russians were no worse than their Western contemporaries in their obtuseness toward the "natives" is based on a fragment of my argument taken out of context. The issue I raised was not the morality of such incomprehension but rather that Russians made policy based

on the mistaken expectation that they would not encounter serious opposition to their territorial claims. Therefore, they were ill prepared to handle the considerable opposition that did in fact arise. In one of the instances cited in the review, the problem of Russian misunderstanding is raised to show that the peace talks with Iran in 1810 failed, even though St. Petersburg wanted an end to this war, because the terms Russia offered as a compromise were perceived by the Iranians as wholly inadequate and insulting (p. 140).

Bias is also alleged in my failure to portray many important Russian officials in the Caucasus as heroes, in contrast to the way they are traditionally represented. In the book I did not deny that Russian and other writers on the subject have often presented the leading figures in the conquest of the eastern Caucasus in a favorable light. It is not my role as a historian, however, to ignore the evidence that shows that portrayal to be inaccurate. The evidence with which I supported my interpretation is not refuted in the review, nor is there any mention of the extensive documentation, often from the papers of Russian officials, on which that interpretation is based. Apparently the only reason for rejecting my argument is that it does not fit the clichés.

The reviewer claimed to have found many factual errors but the only example he cited is a point on which he is in error. He considered it ludicrous that I should have named icebergs as one of the obstacles to sailing the Caspian in winter. Had he checked some scholarly treatise on the Caspian, he would have learned that in the winter that sea is affected by Siberian and Arctic weather systems, which cause the northern part of the sea to freeze solid and produce some freezing elsewhere. In the words of Professor K. K. Giul, "As a consequence of the movement of ice at the end of winter, hills of ice form, reaching a height of 20 m[eters]." Winds carry the ice to many parts of the sea, constituting a hazard even for modern vessels (*Kaspiiskoe more* [1956], pp. 45, 46; also see pp. 32–36, 44).

The review's other criticisms are based on the same kinds of distortions as the ones discussed here. The points raised in the review have only the most tenuous connection with what I really wrote.

MURIEL ATKIN

George Washington University

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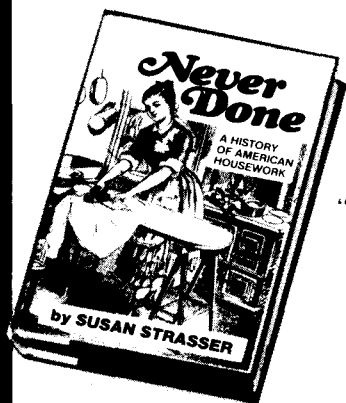
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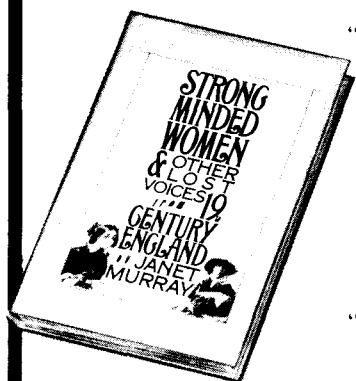
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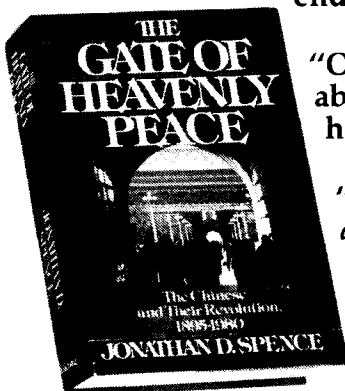
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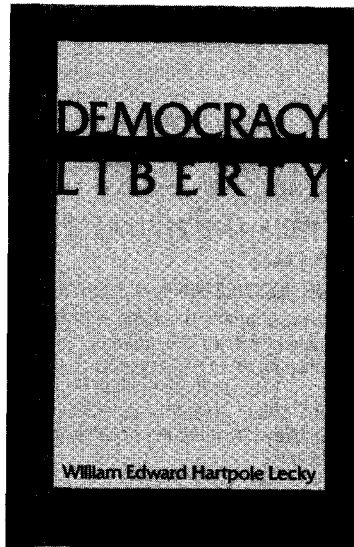
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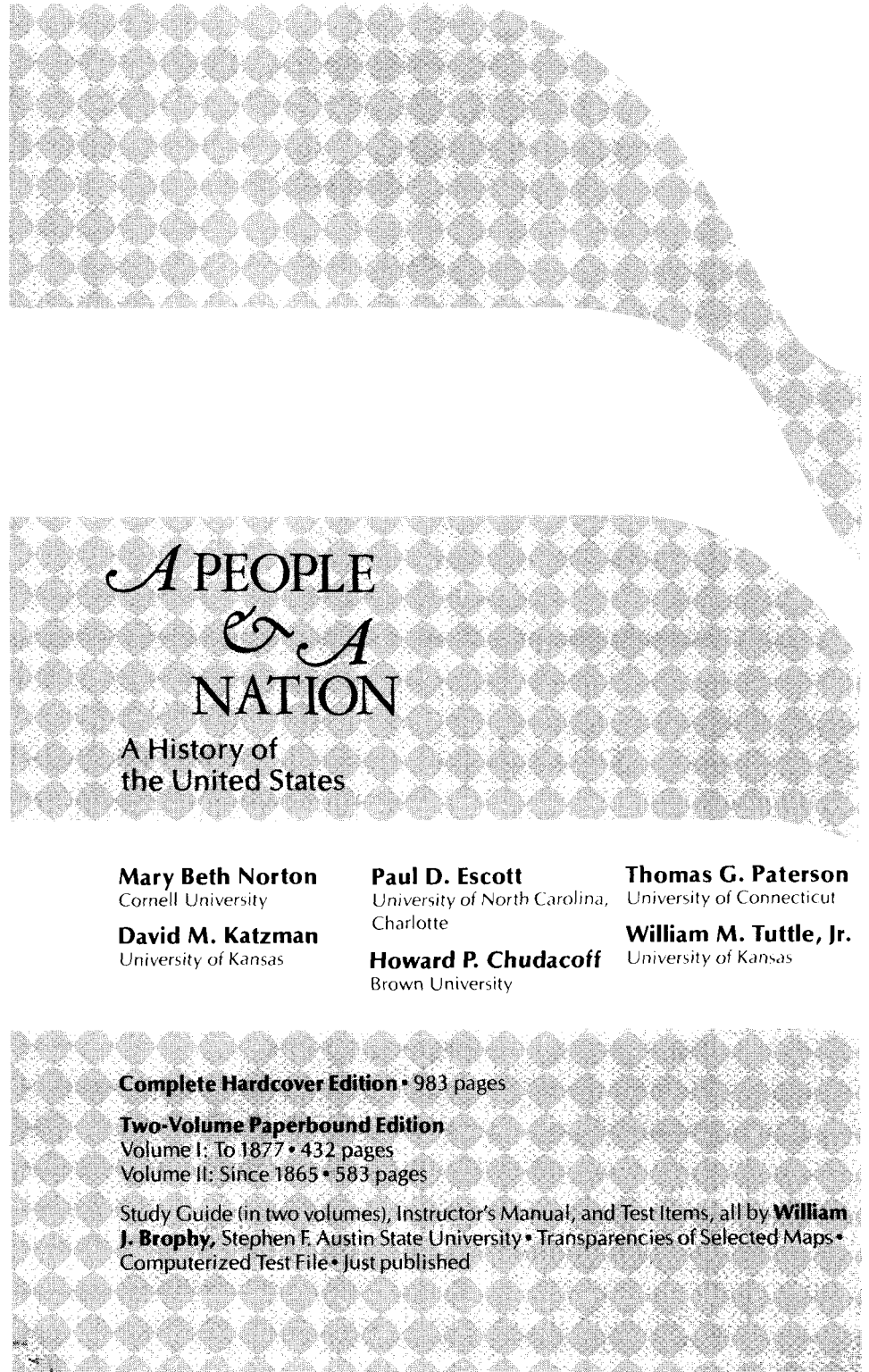


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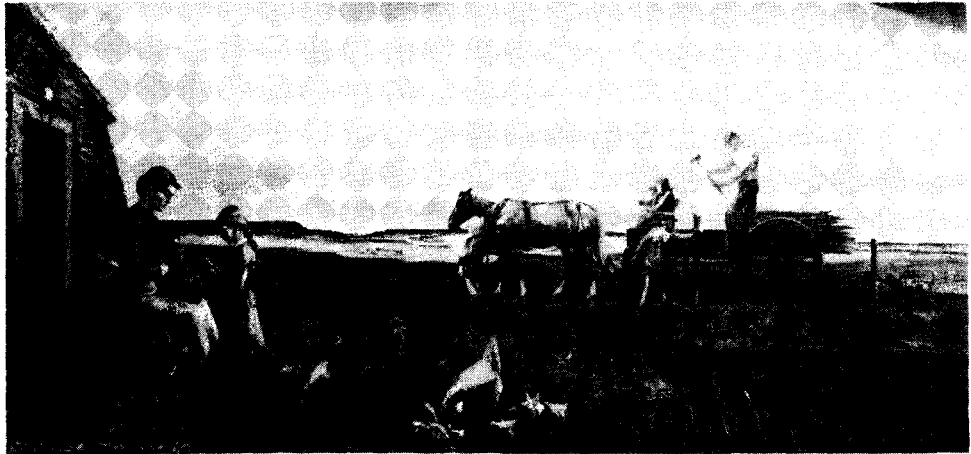
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Index of Advertisers

American Historical Association	32–33	Ohio State University Press	10
Cambridge University Press	18–19, 26	Oxford University Press	30, Cover 2
Columbia University Press	14–15	Pantheon Books	5
The Dorsey Press	12	Penguin Books	21
Harlan Davidson, Inc. (formerly AHM Publishing)	25	Princeton University Press	Cover 4
Harvard University Press	13, Cover 3	University of California Press	28–29
Houghton Mifflin	22–23	University of Chicago Press	17
Indiana University Press	6	University of Nebraska Press	20
Johns Hopkins University Press	27	Viking Press	11
Alfred A. Knopf	7–9	Vintage Books	3
Liberty Press/Liberty Classics	16	H. W. Wilson Company	4
Louisiana State University Press	24	Yale University Press	31

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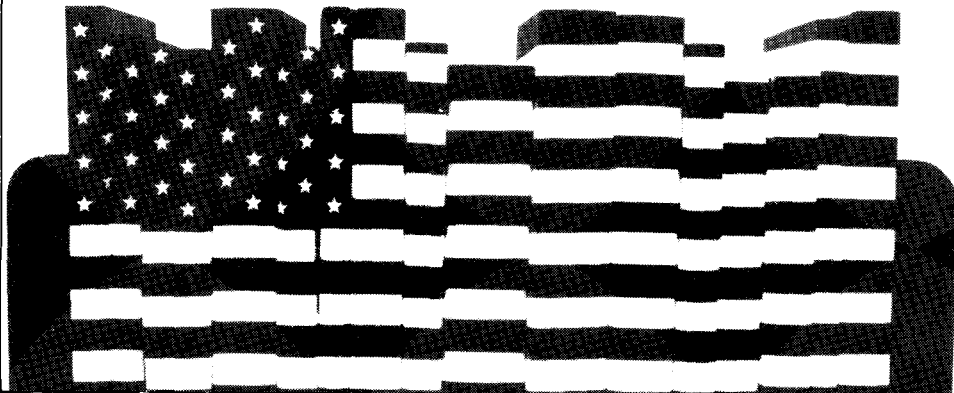
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